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THE MENTAL TEST AS A PSYCHOLOGICAL METHOD [1]

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I

However much one may dislike to 'embark upon the troubled waters of definition,' as President Angell once picturesquely phrased it, it seems necessary to begin by trying to come to some understanding with regard to what a mental test—or, if you prefer, a psychological test—is.

The English term 'mental test' was first used, so far as I can learn, by Professor Cattell in 1890.[2] The tests he described were brief samplings of such abilities as strength of grip, reaction time, color naming, and memory span. A year earlier, in a discussion of intellectual disturbances following upon cerebral injury, C. Rieger made suggestions looking toward what he called '*Eine allgemein anwendbare Methode der Intelligenzprüfung.*'[3] The idea, however, is implicit in an article by Galton published ten years earlier 1879.[4] The earliest formulated definition I find is that of Binet and Henri, in 1896.[5] The method of mental tests, they say, 'consists in the selection of a number of tasks designed to give detailed information on individual differences.' One here

[1] Address of the President, before the American Psychological Association, Madison Meeting, December, 1923.

[2] In an article on 'Mental and Physical Measurements' which appeared in *Mind*, 1890.

[3] Stern, 'Differentielle Psychologie,' p. 89, 1911.

[4] 'Psychometric Experiments,' *Nineteenth Century*, 1879. Republished in 'Inquiries into the Human Faculty,' 1883.

[5] *L'année psychologique*, 1896, p. 464.

notes the implication that other psychological methods do not use 'tasks' and are not intended to give information on individual differences. Later definitions have usually followed along similar lines, but have tended to distinguish rather more explicitly between the mental test and the psychological experiment. Thus Whipple, in the introductory chapter of his 'Manual of Mental and Physical Tests,' 1910, says: "The mental test in some respects resembles, in some respects differs from the typical experiment of the psychological laboratory. Like this latter, the test is superior to the casual observation of everyday life because it is purposeful and methodical; it thus possesses all the merits common to experimental investigation at large, viz: the control of conditions (including the elimination of disturbing, and the systematic isolation of contributory factors), the possibility of repetition, and the possibility of subjecting the obtained results to quantitative treatment. Unlike the typical experiment of the psychological *laboratory*, the mental test ordinarily places little or no emphasis upon introspective observation by the subject, in part because of its relatively short duration, in part because it is frequently applied to inexperienced subjects who are incapable of aught but the most elementary introspection, but more especially because it is concerned less with the qualitative examination or structural analysis of mental processes than with the quantitative determination of mental efficiency; because, in other words, it studies mental performance rather than mental content."

In the 1914 edition of his 'Manual,' Whipple repeats what he said in 1910, but makes a significant insertion. His statement differentiating the test from the experiment now begins: "The primary difference between the research-experiment and the test-experiment is really one of aim. The test has a diagnostic, rather than a theoretical aim: its purpose is not to discover new facts, principles or laws for the science of psychology—though such a result may indirectly be attained—but to analyze, measure and rank the status or the efficiency of traits and capacities in the individual under examination." That is, whereas the distinction drawn in

1910 was based entirely on the nature of the method itself, the distinction of 1914 is based first of all on aim.

This insertion seems to have been influenced by the distinction drawn by Stern in the 1911 edition of his 'Differentielle Psychologie,'[1] which runs as follows: "The standpoint of the test-experiment (*Prüfungsexperiment*), which always characterizes individual psychology, is somewhat different from that of the research-experiment (*Forschungsexperiment*). We give it the English designation 'Test,' in the sense of '*Stichprobe*,' but limit its use to the true test-experiment, although by others the term is sometimes extended to include the research-experiment. A test is thus an experiment which is designed to determine the individual mental character of a person or one of his special mental traits. Its immediate aim is, therefore, not a purely scientific-theoretical one, but a diagnostic one; its purpose is not to discover unknown laws or new relationships, but to classify a particular case with reference to known phenomena. To be sure, the goal of such diagnostic tests is the drawing of conclusions; if possible, conclusions of scientific value. The logical presuppositions of the test are, first, that it shall afford a real indication (*Zeichen*) of a significant trait; second, that in respect to this trait it shall give the individual tested a definite rank in a variational series." "An experiment can only be called a test if, in spite of its acute character, it possesses a chronic symptom-value."

Dunlap, in his 'Elements of Scientific Psychology,' 1922,[2] states the difference as follows: "In mental measurement, the mental capacities and performances of individuals are measured with a view to the comparisons of individuals with one another, either for the purpose of selecting certain types of persons for certain purposes, or else for the rating of these individuals for some other practical ends. In Experimental Psychology, although measurements are always made on individuals, the purpose is to discover and analyze traits common to all of the group investigated, the differences

[1] Pp. 87–88.
[2] P. 19.

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being difficulties to be overcome by experimental technique, rather than being the main objective."

I do not know whether any significance is to be attached to the fact that in the passage just quoted *Experimental Psychology* is printed with initial caps while *mental measurements* is not! A later passage in the same text[1] leads me to suspect that the inconsistency in orthography may not have been accidental. The passage runs as follows: "In certain cases"—the author is discussing the measurement of sensory discrimination—"it is important and possible to make determinations in which small units of measurement are not demanded, and in which the reactor is not required to determine just perceptible and just imperceptible values. Such determinations are called *tests;* and although in each case certain details of technique are indispensable, this technique is neither as detailed nor as difficult of acquisition as the technique of exact measurements." That is, if the measurement is exact, it is an experiment; if only an approximation, it is a test. If such is the implication, I have no reason to suppose that Dunlap is the only Experimentalist of capital E persuasion.

As the above quotations indicate, there is a tendency to stress the following differences between a mental test and a psychological experiment: the mental test is primarily concerned with individual differences, rather than with the universals of psychology; it is applied to a large number of subjects; it studies mental behavior rather than mental content; it makes little use of apparatus; it is intended to accomplish a quick determination of an individual's status with respect to established names; its results, although at times they may have scientific value, are usually less exact than those of the experiment.

Without meaning to imply that all of these distinctions are groundless, I would raise the question whether to the extent that they hold at all, they reflect fundamental and necessary pre-suppositions of the mental test or only the accidents of its history. As for myself, I have never been able to see why

[1] P. 131.

one who employs the test method should confine himself to the study of individual differences; why he should dispense with introspection, if he considers it important for his purpose; why his aim should necessarily be practical; or why his results should be less exact or less capable of verification than those of the experimentalist.

When a few weeks ago I undertook to set down some of the considerations which seemed to me to justify this view, I found myself hesitating. After all, idols of the den are no mere figure of speech, and I realized that, with the exception of a few youthful forays which I made under the tutelage of Bergström and Sanford, I had lived my psychological life pretty much in the test den. I therefore decided to wait until I could take a few soundings; or, to keep the figure more consistent, until I could question the oracles of other psychological dens. My decision finally expressed itself in the form of three questions, which I addressed to twenty-two members of this Association. The first ran as follows: *What are the essential characteristics of a mental test? By this is meant, what (if anything) distinguishes it from any other psychological experiment?* The second question had to do with the contributions of the test method; the third called for a rating of its value in a way presently to be described.

My letter was submitted to two groups of psychologists, which we may designate the 'random' group and the 'interested' group. The 'random' group (random, at least, for the present purpose) included all the eleven surviving presidents of this Association since 1910. In chronological order they are: Seashore, Thorndike, Warren, Woodsworth, Watson, Dodge, Yerkes, Scott, Franz, Washburn, and Dunlap. The 'interested' group was made up of eleven individuals selected because of their contributions to test psychology, and in such a way that they would represent various fields of interest, including industrial, educational, clinical, statistical, and experimental psychology. The names are: Bingham, Boring, Burtt, Cattell, Goddard, Kelley, Kuhlman, Pintner, Thurstone, Wells, and Whipple. No one in either group failed to reply. In response to the first question—what a mental test is, and

how it differs from a psychological experiment—the two groups did not not show any characteristic differences and have therefore been thrown together. The distinctions most often mentioned fall into three classes, which we may characterize in substance as follows:

1. Tests are intended to throw light upon individual differences; the experiment, to establish general principles. (Mentioned by ten.)

2. The test, in contrast to the experiment, is characterized by simplicity, or brevity, or less elaborateness, or the use of paper and pencil instead of apparatus. (Mentioned by nine.)

3. The test has a practical aim, usually individual diagnosis and guidance; it has to do with technology rather than with science. (Mentioned by seven.)

Other distinctions mentioned by one or more include the following: the test does not control conditions and vary the factors as an experiment must; the experiment is an analytic investigation, while the test merely samples functions to ascertain their presence or amount; the significance of a test depends upon the use of norms; the test ordinarily deals with the higher mental processes.

You will note that the distinctions drawn are for the most part in line with the quotations which I have cited from the literature, with perhaps a little more emphasis upon the greater simplicity of test procedure. However, there are five of my correspondents who point out the essential methodological identity of the test and the experiment. These are Boring, Goddard, Thorndike, Warren, and Woodworth. Watson's statement might perhaps justify including him in the same group. I quote their replies.

Boring: "(*a*) Methodologically there is no essential difference between a mental test and a scientific psychological experiment. (*b*) Historically in actual usage there is a difference. I should attempt to summarize the history of the tests as follows: A mental test is an *abbreviated experiment* upon an *individual* in which his *behavior* is observed in order to determine his *capacity* with respect to some *biological use.*

Its motivation is, therefore, biologically and often socially *practical.* The loss of precision, due to its abbreviation, is usually offset by its application to a large number of individuals, so that it becomes a precise measure for a *group.*"

Goddard: "Nothing distinguishes the mental test from any other psychological experiment. The difference is in the use we make of the data."

Thorndike: "There is no sharp line, in my opinion, between what we call a mental test and any other psychological experiment. A mental test usually seeks to measure the amount of some one thing which is supposed to increase in amount without changing its essential nature."

Warren, quoting the judgment of his department faculty: "We are unable to agree on any characterization which will differentiate the mental test from *all* other experiments. As compared with ordinary laboratory research it is objective rather than introspective—but so is the conditioned reflex method, yet the latter is evidently not 'mental testing.'"

Watson: "The first time a test is given to a sufficiently large group to bring out its essential points it is like any other psychological experiment. If repeated it becomes a tool or a routine."

Woodworth: "A distinction some have drawn is that a test appertains to applied psychology, an experiment to pure psychology. This is obviously on the wrong track, since, first, experiments can be and are made in applied psychology (*e. g.* on advertisements), and since, second, the ultimate aim of the investigator who employs tests may be to solve a problem in pure science (*e. g.* mental heredity). Nor could you always tell a test from an experiment by watching the two in progress, since both might use the same apparatus and procedure and give identical data. The difference lies neither in the immediate procedure, nor in the ultimate end in view, but in something in between; namely, in the direct use to be made of the data, whether as general in reference, or whether as individual. "However," Woodworth continues, "what I have said applies to the *use* of the test rather than to the process of standardization and obtaining of norms. The

latter would itself be an experiment, according to my distinction, and would also be a result in pure psychology."

It is the purpose of this paper to try to convince you of the essential correctness of the position which these half dozen psychologists have joined me in taking, though I am unable to accept without qualification all of the statements they have made. As I proceed you may feel that I am emphasizing differences of opinion which are more apparent than real; that the views to which I shall take exception would hardly be open to criticism if one took account of the definitions which they presuppose. In this you would be partly correct; but even so, my problem remains. I wish to show that psychologists have too often conceived of the mental test as a mere practical device; that it has a large value as an instrument of research; and that its kinship to other psychological methods is much closer than either the average tester or the average laboratory worker appears to assume. First, let us examine further the grounds of the distinctions which have been drawn.

II

1. *Use of Tests in Individual Psychology.*—That mental tests are the method *par excellence* of individual psychology does not mean either that they may not be used for other purposes or that individual differences may not be studied by other than test methods. That there exist quick-determination tests which can be applied to large numbers does not mean that tests of more searching character may not profitably be carried on with as small a number of subjects as you please. Binet's three year mental test study of his two daughters shows how unessential to the method large numbers are. On the other hand, that experimental psychology has usually dealt with so few subjects may be due less to deliberate choice than to the natural limitations imposed by its technique, and to its early relationships with physiology, a science which has always worked chiefly with small numbers. The view that in experimental psychology individual differences are to be regarded as 'difficulties to be overcome by experimental technique,' seems to me indefensible if

psychology is conceived as a science of all the facts rather than of part of them. It harks back to the ancient confusion of errors of observation with supposed errors of nature in creating types or realizing an ideal. Experimental psychology is finding, just as physiology is finding, that the neglect of apparent departures from the norm is likely to result in a totally erroneous conception of the norm itself.

2. *Pencil and Paper Character.*—In the little use it makes of apparatus the test is not alone. A fairly large number of investigations in the experimental field have been made in the last twenty years with no other aids than pencil and paper, nor is the practice becoming less common. On the other hand, fairly complicated apparatus is sometimes used for test purposes; for example, in tests of flying ability or of honesty of report.

3. *Omission of Introspection.*—Although resort to introspection in the use of mental tests is exceptional, this fact offers no basis for a distinction between a test and an experiment, unless we wish to classify all behavior methods as tests. The historical fact is that subjective and objective methods in psychology have existed side by side since the very beginning of the science. Weber and Fechner laid its foundations by methods which were rather more behavioristic than introspective, in any true sense of the term. The objective investigation of memory by Ebbinghaus was undertaken in 1879, the very year in which Wundt established at Leipzig the first psychological laboratory. From the middle eighties on through the nineties objective experimental work was carried on very actively by the psychologists of Germany, France, and America. And, just as objective and subjective methods grew up side by side in experimental psychology, so in mental tests have both methods been employed; for example, by Binet,[1] Sharp, Meumann, and Whipple. That the practice has not been more general is of course due in part to the frequent use of tests with subjects for whom introspection is out of the question; but over and above this is the

[1] Binet, in his earlier years, was even ready to rule out of psychology any kind of experimental work which did not make use of introspection. ('Introduction à la Psychologie expérimentale,' p. 18. 1894.)

fact that mental tests represent one aspect of a fairly widespread movement away from the atomistic psychology of Wundt and his immediate followers.

4. *Exactness, Verifiability of Results, Control of Conditions, and Possibilities of Analysis.*—As affording a cross-section picture of the individual's efficiency in this or that trait, or of the relationship obtaining among given traits, the accuracy of the test result depends, if the usual precautions of experimental procedure have been observed, entirely upon the appropriateness and adequacy of the sampling which the test affords. The sampling can of course, be made as extensive as may be necessary; how extensive it needs to be, is always a matter for statistical determination. This holds not only with respect to sampling for the diagnosis of 'acute' conditions, but also with respect to sampling for 'chronic' traits. That the latter could be revealed by any ordinary amount of sampling was long beyond the hope of many psychologists. However, as validity data are showing, some of our tests are diagnostic of chronic traits to a high degree. A fifty-minute intelligence test of high school freshmen yields as valid a prediction of the marks these pupils will receive five years hence in a university, as is yielded by their scholastic performance in high school during four years of a thousand hours each. The validity of our best tests holds not only for a group, as a group, but also, to a considerable extent, for the individual.

Whether these 'chronic' traits reflect primarily the influence of endowment or of environment, is a question to which no certain answer can at present be given. As far as intelligence is concerned, there is no convincing evidence that such differences as we find among children of a given nationality who have had the ordinary advantages offered by the public school and by other common social contacts are due, in any considerable degree, to environmental factors. That there is no possibility of creating high I.Q.'s artificially is a proposition which, like any other universal negative, is not open to experimental proof. On the other hand, if the I.Q. *is* largely the product of environment, it should be easy to prove the

fact beyond cavil. The oft-quoted proof offered by Cyril Burt that Binet scores are, to the extent of three-fourths, due to other factors than intelligence loses all significance when we note that by 'intelligence' Burt means nothing more than scores on his syllogism test. It will be better evidence if someone can show that foster sibs (of unknown parentage) adopted early resemble each other or their foster parents as much as true sibs resemble each other or their true parents; or if it can be shown that by special treatment from an early age the I.Q.'s of a group of fifty or a hundred children can be raised significantly above the I.Q.'s of their untreated sibs. The issue here is one of the most important humanity faces, and one that will some day be decided on the evidence of mental tests.

With regard to the control of conditions and the verifiability of results, the test psychologist can point to numerous published arrays of probable errors and coefficients of reliability. These are not always as he would have them, but that he does have them is the significant fact. I am not aware that anyone has ever computed a probable error of an introspective report. In the use of any standardized psychological test, different investigators can readily control the conditions sufficiently to get practically identical results. At the opposite extreme is the condition depicted by one of my friends, a man who has made notable contributions to introspective psychology, that probably not more than a half dozen individuals in this country are able to give a dependable introspective account of their mental experiences. Possibly my friend is unduly pessimistic, but we are all aware of the difficulties which the introspectionist encounters because of the influence of expectation, interest, and suggestion. Whether, for example, a given observer finds that he has imageless thoughts, depends entirely too much on whether imageless thoughts are supposed to inhabit the particular laboratory in which he works.

To the test psychologist, the most exasperating thing about the average published introspective report is its indefiniteness. If you met it five minutes afterwards 'on the

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street' you would not recognize it. Perhaps, as in Messer's experiments, the observer is trying to describe his mental experiences provoked by the stimulus question 'Is eternal peace possible?'[1] One might report that he saw himself in a graveyard; or heard a solemn voice that he associated with Jesus; or experienced an æsthetic appreciation of the poetic value of the phrase 'eternal peace'; or that he felt himself inclining the head in attitude of affirmation. Now if we were to translate mental test scores into this kind of description we should have something like this: 'Saw an analogy relationship'; 'a word occurred to me that was the opposite of something'; 'solved a problem that had some numbers in it'; 'appreciated the resemblance between a funny shaped block and an inset in a board'; etc. Imagine the test psychologist trading his objective scores for this kind of evidence, especially if he found that the stimulus which aroused in one observer the analogy experience, aroused in another the arithmetical or form board experience!

The contrast between objective test and subjective observation is in some respects like the contrast between phototelescopic and oculotelescopic study of the stars. The phototelescopic method, like the mental test, gives a permanent objective record which can be studied at leisure and subjected to various kinds of quantitative treatment that cannot be applied to the fleeting visual impression. It is largely freed from the influence of the personal equation.

It may be objected, however, that the exactness of the test score, even apart from any question as to the soundness of its statistical presuppositions, is illusory, because it stands for an unanalyzed complex of Heaven knows what contributing elements; that the attempt to test intelligence, or mechanical ability, or moral judgment, or psychopathic predisposition in our present state of ignorance of what these traits are is rather worse than folly; that it is the business of experimental science to isolate factors, and that this is something which the test method fails to accomplish.

Certainly no one can deny the difficulties the psychologist

[1] 'Experimentalpsychologische Untersuchungen über das Denken,' *Archiv f. d. ges. Psychol.*, 8, p. 41, 1908.

encounters in meeting all the conditions of ideal experimental procedure. But are these difficulties peculiar to the test method? I do not think they are. That investigators of animal learning, for example, have not succeeded in ruling out all disturbing factors is shown by the relatively low correlations which have been found for learning records made by the same animals in different series of experiments.[1] Fortunately, in this case, the very fact that reliabilities of learning scores are obtainable affords a guarantee that whatever refinement of procedure is necessary will ultimately be attained. Whether introspection holds out any such hope is a question. In the experiment of Messer to which I have referred, the uncontrolled and uncontrollable factors are so many that there is no predicting what conscious content will be produced if the stimulus is repeated a second or third or fourth time.

In reply to the psychologists who insist that we should first define what we would attempt to measure, one might retort by demanding a generally acceptable definition of a few 'simple' terms like *sensation*, *feeling*, *attitude*, or *instinct*. This would not dispose of the problem, but it ought to lend a touch of mutual forbearance, if not of sympathy. It may be pointed out, furthermore, that the test method is by no means lacking in technique for ascertaining what factors contribute to a score. I refer especially to the method of partial correlation, which is probably as analytic as any technique the laboratory psychologist is accustomed to employ. In other words, the test itself will furnish ultimately the definitions which our critical friends are so impatient for. One might add, in passing, that the partial correlation method and the method of correction for attenuation would prove as valuable in the experiment as in the test, provided large enough numbers were used to permit of generalization.

5. *Practical vs. Theoretical Aim.*—Here, some will say, we have at last a valid basis of distinction. It is the method of tests that has brought psychology down from the clouds and

[1] See Wm. T. Herron and Walter S. Hunter, 'Studies of the Reliability of the Problem Box and the Maze with Human and Animal Subjects,' *Comp. Psychol. Monog.*, 1, 1922.

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made it useful to men; that has transformed the 'science of trivialities' into the 'science of human engineering.' The psychologist of the pre-test era was, to the average layman, just a harmless crank, but now that psychology has tested and classified nearly two million soldiers; has been appealed to in the grading of several million school children; is used everywhere in our institutions for the feeble-minded, delinquent, criminal and insane; has become the beacon light of the eugenics movement; is appealed to by congressmen in the reshaping of national policy on immigration; is furnishing high-powered explosives for the social reformers of one wing, while serving at the same time as the target drawing the hottest fire from the other wing,—no psychologist of to-day can complain that his science is not taken seriously enough. And is not most of this change, you will say, due to the mental test?

It is true that the practical usefulness of mental tests is one of their outstanding characteristics. This was recognized long before the first psychological laboratory was established, as is illustrated by Solomon's test of the rival claimants for possession of the disputed child (a group test of the maternal instinct!); or by the ancient ordeal that required the accused to prove his innocence by salivating and swallowing mouthfuls of desiccated rice (test of honesty of report by the conditioned reflex method!); or by the tests that primitive tribes have sometimes employed in the selection of a chieftain (test of leadership). In all of these the practical aim is evident enough. It is no wonder that the average individual, especially since he himself has from the earliest childhood used tests as a means of increasing his knowledge of those around him—applying a stimulus or setting a condition to see how another would react to it,—can see nothing in a modern psychological test but a useful device.

However, when one looks into the history of tests over the last thirty years, one finds that the psychologists who devised and used them were by no means always governed by practical considerations. Admittedly, one is often at a loss

to unravel the motives which have actuated his fellow scientists, or for that matter even himself, and I cannot be sure that you would agree with me in regard to any particular individual. I do believe, however, that almost anyone will, by a careful study of the literature of test psychology, be led to the conclusion that a considerable number, if not a majority, of the psychologists who have worked with tests, have had as much of a theoretical as a practical interest in the outcome. For example, I would so classify Binet, Cattell, Spearman, Stern, and Thorndike.

The inclusion of Binet's name in this list may be questioned, but the history of his life and work justifies it. His interest was in the science of psychology. His intelligence scale was in a sense a by-product, even if all his earlier work did contribute toward it. In the four years he lived after completing the 1908 scale he made not a single application of it to practical problems and attempted no really serious revision. By 1911 he had moved on to new fields. Had he lived he would no doubt have continued to use tests, but he would have used them primarily for purposes of research. In the account of his extended test study of his two daughters he suggests no applications of this type of mental analysis, but is interested rather in the elucidation of the functional and dynamic point of view in general psychology.[1] One sees the same thing again in his 1909 article, 'L'intelligence des imbeciles.'[2] As the last twenty-five pages of that article show, Binet was far more interested in imageless thought than in imbeciles. His work heads up in a scheme of the higher mental processes, rather than in any scheme of segregating or teaching the mentally defective.

Stern's 'Individual Psychology' is only an approach to a systematic psychology of personality. Mental tests as such he at first had little use for. At the 1904 Congress of Experimental Psychology in Giessen, commenting on a statement by Henri which he misinterpreted to mean that Binet and Henri had given up mental tests, Stern expressed satisfaction that these investigators had at last abandoned a method

[1] 'L'étude expérimentale de l'intelligence,' 1903.

[2] *L'année psychologique*, 1909.

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which could 'bring nothing but discredit upon scientific psychology.' Spearman no one would accuse of neglecting the theoretical implications of test results, and I think the same could be said of Thorndike and Cattell. And is not this true, in varying degrees, of most of us? Certainly many of the recent contributions by members of this Association would suggest far less interest in the diagnosis of cases for practical purposes than in questions of such general interest to psychology as mental development, mental inheritance, I. Q. constancy, race and sex differences, and the nature of intelligence.

Some would lay down the principle that investigations which use tests belong to experimental psychology as long as they are in search of fundamental laws, whether these are sought for their own sake or in order to establish a basis for improved methods of testing; but that when the end-product of such experiments—the perfected test—is put to use we no longer have to do with experimental psychology or with psychology at all. From this point of view, the mental test might be defined as 'canned science,' and would be typified by the Seashore phonograph records.

It must be admitted that when the test has been perfected, and, I am afraid, sometimes before, hordes of untrained workers are likely to use it for all kinds of purposes that have nothing to do with science. The fact remains, however,that if the test has been properly constructed and validated it may at once become a tool for the investigation of important scientific problems, such as mental growth, educability, individual differences, or mental organization. *It is only because tests are capable of practical application that we overlook their research possibilities and come to think of them as something to be contrasted with other methods of psychological investigation.*

After all, who can distinguish between the theoretical and the practical or draw a clear line between pure and applied science? As Watson has pointed out, the fact that a psychologist has discovered explanatory facts and principles capable of ready application does not make him an applied psychologist; and on the other hand, the mere fact that one is

moved by practical motives is no evidence that his work may not prove of capital importance for the advancement of his science. It was in researches on such homely, practical problems as chicken cholera, swine fever, rabies, the diseases of swine, the diseases of silkworms, and the manufacture of alcohol and vinegar that Pasteur created out of hand the science of bacteriology. The enormously rapid development of theoretical chemistry, electrical science, and histology under the stimulus of human needs is a no less significant object lesson. A sufficiently keen realization of the fact that a full-grown science of psychology would enable us to add to the efficiency, happiness and morality of mankind, perhaps even to his average native endowment, and an abiding faith that these ends are actually attainable, might in the long run carry us farther toward the goal of pure science than millions for research without the dynamic effect of these incentives.

III

If time were available to recount the history of mental tests it would be easy to show how intimately the growth of test psychology has been connected with that of experimental psychology generally. The one is almost as old as the other, and the names associated with the two lines of development are in large part identical. For example, one can hardly say whether Galton is more connected with the history of experimental psychology by his study of mental imagery, published in 1880,[1] than he is connected with mental tests by his 'Psychometric Experiments' published in 1879.[2] In the latter paper Galton tells how he made out a list of 75 words and used them in a free-association test. His method was to expose a word and then write down the first two words it suggested. What a little step to Jung, to Kent and Rosanoff, and to the Wyman association test of interests! And if the name of Ebbinghaus were used in such an experiment with psychologists as subjects, who can say whether the response frequency of *nonsense syllables* would be greater or less than that of *completion test?* Kraepelin, who must be named in any

[1] In *Mind.*

[2] *Nineteenth Century.* Republished in 'Inquiries into the Human Faculty,' 1883.

history of experimental psychology, worked chiefly with tests. That he confined himself to tests of the simpler processes may have been due in part to German caution, but it was more probably due to the experimental traditions engendered by Weber, Fechner, and Wundt. In his lively and prophetic introduction to the first volume of his *Arbeiten*, in 1894,[1] he made it clear that he looked forward to the time when it would be possible to make 'a comprehensive determination of every kind of mental process.'

In the land of Charcot, Ribot, Janet, and Binet, psychological interests centered about the larger problems of mental integration and personality, and as a result, mental tests in France, almost from the beginning, dealt with the more complex mental processes. This background colors the entire work of Binet, as one can see in his book, 'Psychologie des grands calculateurs et joueurs d'echecs' and in his article, in *Revue philosophique*, 'De la suggestibilité naturelle chez les enfants,' both published in 1894; in his two articles, 'La psychologie individuelle,' 1895,[2] and 'La mésure en psychologie individuelle,' 1898;[3] and in his book, 'L'étude experimentale de l'intelligence,' 1903.

In America, Cattell founded our first psychological laboratory and gave us our first series of mental tests. In him the Wundtian and the Galtonian influences combined in a way to put the science of individual differences on its feet and to give the mental test movement in America an auspicious beginning. For twenty years the test was cultivated in this country primarily as a method of experimental psychology. One recalls the correlational studies based upon Cattell's tests of Columbia students;[4] Bolton's report of memory tests given to Worcester school children in 1891;[5] Muensterberg's

[1] Vol. 1, p. 43.

[2] *L'année psychologique*, 2, 411–465.

[3] *Revue philosophique*, 1898.

[4] See Cattell, 'Mental and Physical Measurement,' *Mind*, 1890, 373–381; Cattell and Farrand, 'Physical and Mental Measurements of Students of Columbia University,' PSYCHOL. REV., 3, 618–648, 1896; and Wissler, 'The Correlation of Mental and Physical Tests,' PSYCHOL. REV. MONOG. 3, 1901.

[5] 'Growth of Memory in School Children,' *Amer. J. Psychol.*, 4, 362–380, 1892.

series of ten tests described in 1891;[1] Jastrow's extensive preoccupation with tests in the early nineties, and especially his article on the 'Community of Ideas in Men and Women,' 1894;[2] Miss Calkins' article of 1896 on a repetition of this test;[3] and the interesting contribution by Royce the same year describing a test of inventive ability.[4] These are but samples.

Of the thirteen presidents of this Association since 1910, I think that all but two have worked with mental tests, and that even these have sponsored dissertations based upon test data. Of the nineteen presidents of the Association from its founding in 1892 up to 1910, the names of nine would belong in any reasonably complete bibliography of mental tests. The founder and first president of the Association, to whom so many of us are indebted for priceless inspiration, seems to have been the first in America to use a mental test for purposes of research. I refer to Hall's study made in the fall of 1880 on 'The Contents of Children's Minds,' a study which may be regarded as the lineal ancestor of our many present-day tests of information and vocabulary.[5] Some of you may share my regret that so 'intrepid a devotee to concrete human nature,' to use Thorndike's apt characterization, should not have turned his attention to the refinement and extension of the promising method with which he began.

This Association previous to fifteen years ago had had two committees on mental and physical tests. The first was appointed at the 1895 meeting on the suggestion of Baldwin, and included Jastrow, Sanford, Witmer, and Cattell (chairman). The recommendations of this committee were presented at the 1897 meeting by Jastrow and Cattell.[6] In a discussion of this report Baldwin described three memory tests which he had devised. The second committee was

[1] 'Zur Individualpsychologie,' *Zentbl. f. Nervenheilk. u. Psychiatrie*, 1891.

[2] PSYCHOL. REV., 1, 152, and PSYCHOL. REV. 3, 68–71.

[3] PSYCHOL. REV., 3, 426–430.

[4] 'The Psychology of Invention,' PSYCHOL. REV., 5, 111–143.

[5] *Princeton Rev.*, 1893. This is a refinement of an experiment carried out in Berlin in 1870. For an account of the many repetitions of this experiment see B. Hartman, 'Die Analyse des kindlichen Gedankenkreiss,' fifth edition, 1909, pp. 61–139.

[6] PSYCHOL. REV., 1898.

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authorized in 1906 and was called the Committee on Standardizing Procedure in Experimental Tests. It consisted of Judd, Pillsbury, Seashore, Woodworth, and J. R. Angell (chairman). The committee's report of 1910 contained an article by Pillsbury, on 'Methods of Determination of Sound Intensity'; one by Seashore, on 'Measurement of Pitch Discrimination'; and one by Angell, on 'Determination of Mental Imagery.' Yerkes and Watson, who were invited to work with the committee, prepared a monograph on tests of color vision in animals; Woodworth and Wells in collaboration produced their well-known monograph on tests of association.

We see, therefore, that historically the mental test grew up as a fairly integral part of experimental psychology. One must not be misled by the fact that it has often been cultivated for its immediate usefulness, and that its problems have compelled it to develop a technique of its own. Objectively, the psychological test is a method of sampling mental processes or mental behavior, and so, in a sense, is any method of psychological experiment. The investigations of animal intelligence have been based almost entirely upon what might legitimately be called the test method; have, indeed, often used the same kind of material as is used in mental tests, such as the maze, the puzzle box, and the multiple choice apparatus. The present-day animal studies are coming more and more to resemble the test-experiments, and to deal with such problems as individual differences, mental inheritance, and the interaction of mental functions. Both with human and with animal subjects, experimental investigations of memory are samplings of memory ability; those of skill acquisition are samplings of habit formation; those of attitude, instinct or will are samplings of these mental and behavior phenomena.

As regards the complexity of processes studied, we have seen that both the test and the experiment show a fairly wide diversity of practice from their earliest beginnings to the present. On the whole, perhaps one can say that for a decade or so the simpler processes played in the test a rather predominant rôle, and that during the last quarter century

the situation has been reversed.[1] For example, Oehrn's article on tests of individual differences, first published in 1889,[2] gives the rank orders of his ten subjects by name, not omitting himself and his teacher, Kraepelin. The psychological student of today may well lament that test literature no longer possesses this element of biographical interest, but the fact that it does not is eloquent tribute to the increasing success of mental tests in affording a picture of the subject's '*innere Persönlichkeit.*' But if the test somewhat more quickly than the laboratory experiment moved on to the more complex processes, the experiment was not far behind. The invasion was part of a general movement in which all psychology shared. Binet in 1909[3] calls attention to the fact that he first set forth his ideas on the 'scheme of thought' in 1900, and intimates that they were taken up by the Würzburg school without due credit to their source. Whatever the extent of this debt, one can say that for some fifteen years now experimental psychology in Germany, like the mental test everywhere, has concerned itself chiefly with the higher thought processes. One would probably be safe in predicting that the next decade will see a large interaction between the psychology of mental tests and the experimental psychology of thinking. On the one hand, the opening up of problems of individual differences by means of tests will inevitably lead to the more intensive study of such differences by the usual laboratory methods; and, on the other hand, the success attained by tests in the diagnosis of abilities for useful purposes is likely in turn to have a considerable effect upon the technique of experimental psychology. Woodworth, for example, has shown how a simple test of memory for paired associates may challenge the whole doctrine of association by contiguity, and how a test of incidental memory may cast doubt upon the testimony of trained introspectionists as to the sensory nature of their recalls.[4]

[1] The terms 'simple' and 'complex' are of course used in a relative sense, as we now know that no mental process is really simple.

[2] Five years later republished in Kraepelin's '*Psychologische Arbeiten*,' Vol. 1, 1894.

[3] 'L'intelligence des imbeciles,' *L'année psychologique*, 15, p. 143.

[4] 'A Revision of Imageless Thought,' PSYCHOL. REV., 22, p. 11 and 18 ff. 1915.

IV

I think we may conclude that the attempt to distinguish between the method of tests and the method of experiment is not warranted either on logical or historical grounds. The mental test is part and parcel of experimental psychology. How important a part, only the future can tell. Nevertheless, there is an abundance of opinion, which it may be interesting to sample. A census of opinion may not carry the stamp of finality, but it does divest the issue of individual bias and is the nearest approach we can make to the probable facts.

You will recall that the letter which I sent to twenty-two representative psychologists called for a rating, on a crude seven point scale, of the value of the test method to psychological science. The task given to each was to check one of seven given statements, which ran as follows:

1. *Value less than zero (more a liability than an asset).*
2. *Value zero.*
3. *Value positive, but slight compared with that of other accepted methods.*
4. *Value considerable, but distinctly less than that of other accepted methods.*
5. *Value compares well with that of other accepted methods.*
6. *Value greater than that of most other methods.*
7. *The most important (or promising) psychological method.*

In the 'random' group presidents of the Association since 1910, seven checked the statement 'value compares well with that of other methods'; one, 'value greater than that of most other methods'; three, 'value considerable, but distinctly less, etc.' In the 'interested' group contributors to test psychology, six checked the statement 'value compares well, etc.'; one, the statement 'value distinctly less than etc.'; one, 'value greater than'; and three, 'the most important (or promising) psychological method.' The 'interested' group, therefore, gives somewhat higher ratings, but the mean rating of the two groups combined expresses an opinion slightly more favorable than the statement 'value compares well with

that of other accepted methods,' five rating the test higher than this and four lower.

The above figures at best can but serve to show a trend of opinion among fallible judges. They harbor a certain degree of ambiguity due to the fact that those who replied did not all define 'mental test' in the same way. Naturally, those who gave a larger connotation to the term tended to give the highest ratings. However, making due allowances, it is evident that in the opinion of these twenty-two judges the mental test has come to occupy an important place in psychological methodology. If we could have a similar expression from all the psychologists of the last twenty or thirty years, it is possible that the scattering of ratings would be greater. One can easily imagine that for Wundt mental tests must have held even a lower place than what he called the '*Scheinexperimente*' of Marbe and Bühler. On the other hand, Spearman regards them as 'the most alive and futureful shoot of all contemporary psychology.'[1]

At this point I am reminded that a good many of you harbor a profound distrust of the 'verbal report' and would probably prefer to have a verdict based upon a census of 'behavior.' Very well. In the 1923 Year Book of our Association I find some 362 members recorded as carrying on psychological research. Of these, 147, or 40 percent, are doing research in the field of tests. The actual number, however, is much greater than this, since 94 others, or 27 percent of all, are doing research in a field in which the use of tests is almost inevitable, such as clinical, educational, or applied psychology. Probably the total number working with tests is somewhere between 50 percent and 65 percent of our psychologists engaged in research. In the same Year Book there are 117 members who have received the degree of Ph.D. since the beginning of 1915 and are recorded as carrying on psychological research. Of these, 53 percent are doing research with tests; and, exclusive of these, 30 percent more are carrying on research in educational, clinical, or industrial psychology. Of the 34, or 30 percent, who are recorded as doing research

[1] 'The Nature of Intelligence and the Principles of Cognition,' p. 35. 1923.

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in experimental psychology, all but 14 are also working with tests or in a field that involves the use of tests. The trend is too evident to require comment.

Another sampling of performance may be had from the programs of our annual meetings. I have examined that of the last meeting I attended—1921. That year, so far as I could make out, 33 papers were presented which summarized data of original investigations. Of these, about twenty, or 60 percent, were based either wholly or in part on the test method.

The above figures are not offered in the spirit of a patent-medicine testimonial. It must be admitted that their meaning is far from clear. Some would say that they merely reflect popular demand and present-day professional opportunities. I doubt the correctness of this interpretation. Anyone who has canvassed our universities within the last five years to find suitable candidates for an instructorship or assistant professorship in experimental psychology will probably agree with me that in this field the supply is less adequate to the demand than in the field of tests and measurement. I am inclined to attribute a good deal to the relative human interest of the problems in the respective fields, and to the relative promise they are deemed to offer with respect to significance of results. If such is the motive, it is conceivable, of course, that it rests upon an error judgment. Whether the scientific value of test contributions at the present time is in full proportion to the labor expended upon them, is a question on which there may well be a difference of opinion. Certainly the value is less than it might be, and less than in time it will be.

That the labors thus far have not been entirely fruitless, however, is indicated very definitely in the statements made by our twenty-two psychologists in reply to question 2 of my circular letter: *Have mental tests opened up, or thrown light upon, any problem or problems of substantial importance to psychology as a science?* Nineteen of the twenty-two replies received to this question were affirmative without qualification, and the three exceptions were evidently based upon a more limited definition of the test than was presup

posed by the others. The greatest number (thirteen) mention individual differences, including differences due to age, race, and sex. Eleven say that mental tests have thrown light upon such problems as mental organization, mental types, the nature of intelligence, and the intercorrelations of mental traits. Other contributions mentioned by two or more have to do with heredity and endowment; personality, character, and emotional traits; the higher thought processes; fatigue and other factors which influence efficiency; mental development; the function of language in thinking; educability; the rôle of intelligence in every day life; and, finally, the use of the test as a supplement to, or as an introduction to, more intensive forms of experimentation. As stated by Cattell, "Psychology as a science consists *largely* of the knowledge obtained by quantitative psychological tests. This knowledge is in itself of substantial importance and is interrelated with the whole field of psychology. It is as ultimate, both as description and in its application, as any other part of psychology."

In conclusion, I do not believe that anyone interested in the whole science of psychology can weigh lightly such problems as individual and race differences, the interrelation of mental traits, the phenomena of mental growth, the limitations of educability, or the psychology of genius, mental deficiency and insanity. Upon all these, the mental test has thrown light. It is proving itself applicable not only to the problems of intelligence, but also to those of emotion, volition, temperament, and character. It has become one of the important methods of psychological research; some would say, the most important. Not the least of its contributions is the fact that it has broadened and intensified our incentives to research, enlarged the public support of our science, and attracted new hosts of workers to the psychological vineyard.

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BEHAVIORISM AND BEHAVIOR, II

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The Human Response

As a basis for the discussion which is to follow, two definitions are here submitted:

I. A response is a unified group of muscle contractions which, as movements, form the basis of the coöperative receptor-effector interchanges between individuals.

II. Human behavior is the totality of the response systems that establish the individual's status [1] in the social organization of which he is a member.

Under human behavior I include the educational, vocational, administrative, recreational, and personal activities of the individual and a human response is one of the elements of one of these categories.

To anticipate the criticism that I am merely substituting the terms behavior categories, social organization, etc., for the psychical entity which I claim to repudiate, I wish to affirm that I hold all forms of social activity or achievement as ultimately reducible to electron-proton interactions that are just as mechanistic as any physical or chemical process. The essential characteristic of the social category is the fact that (*a*) the neuromuscular character of a response is relatively unimportant as compared with its effect as a stimulus for other individuals; (*b*) the physical units of measurements as represented by calories, watts, foot-pounds, etc., are relatively inadequate to measure this stimulating effect; (*c*) the individual is classified not on the basis of physical or physiological properties, but on the basis of his coöperative effectiveness in the social organization of which he is a unit.

In this I agree with Warren (**10**) in excluding 'neural activity' from a definition of behavior but I am one of the

[1] In my article 'Behavior and the Central Nervous System' (**12**) the concept of social status is developed at greater length.

behaviorists who 'seem loath to recognize the importance of this neural-adjustment process.' Not because I "fear that it implies the existence of an 'initiating consciousness' in some disguised form," but because I do not see how even perfect neural insight could reveal say the development of such a behavior situation as is involved in the conception of 'democracy.' For me Warren's 'adjustment process' is a social process, not dependent on a specific "*molding of the response to the stimulus* by means of the central nervous activity" (p. 487), nearly so much as upon the social stimuli which form the individual's environment. I do not wish to imply that the behaviorist should not study the nervous system, but neurology cannot explain even the simplest of the social interactions until a *biosocial* analysis has been made.

Classification of Responses.—No attempt will be made here to form a systematic classification of the human responses, but my efforts will be directed more toward indicating the way in which behavioristic psychology can take its place with the physical sciences and assist in establishing the 'social' sciences on a scientific basis. We have seen that the human responses are either biophysical or biosocial. They conform to the strictly physical or mechanistic causal categories without the justification for the introduction of a non-biological or psychical factor. Out of this, three classifications arise: (*a*) The biophysical responses which represent the terminal points of the strictly biological sciences and in which the sensorimotor functions of man are studied as anatomico-physiological phenomena. (*b*) The implicit responses in which those obscure sensorimotor functions that occur within the individual and which he alone can designate or describe are studied. (*c*) The biosocial or overt responses which represent the beginning or the fundamental basis of the social sciences, in which man is regarded as an element in a social organization and in which his responses are studied with respect to their coöperative effectiveness and as stimuli for other individuals.

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The Biophysical Responses

This class includes the sensory-cerebro-motor structures and processes, and since these present no special difficulties for behaviorism, a mere listing will be considered sufficient for the purpose of this discussion. They represent, with certain extensions, the so-called 'muscle-twitch' phase of human behavior.

The list of biophysical components includes the sense organs, the peripheral, connecting, and motor neurons; the contractile and secretory effectors; and the physiological processes that accompany the functioning of these structures. Certain groupings of structures and processes form more or less clearly defined mechanisms:

1. The sensory accommodatory and bodily posture mechanism; *e.g.* listening.
2. The organic regulating mechanism through which the internal bodily metabolic conditions are modified to conform with the energy requirements of the skeletal musculature; *e.g.* respiration rate, blood pressure, vasomotor adjustment.
3. The inherited mechanisms; *e.g.* reflexes, so-called instinctive[1] behavior, family traits, etc.

[1] In connection with the controversy on the nature of instincts, the number, criteria, or even whether there are any, I think we are losing sight of the fact that the term instinct is only a name for a classification. The question is not 'Are there any instincts' but what forms of movements shall we *call* instincts provided the historical antecedents of the term justify its use. We may conclude that the term instincts is not a good one for any classification that will be of service in the investigation of human behavior (a conclusion toward which I lean). In such event the term instinct might just as well be eliminated altogether without in any way effecting any observations that have been established. It seems to me that the present controversy is merely the old one of what is inherited, what is acquired. The zoölogists have had their troubles with this problem and it is not settled yet. The tendency seems to be to drop the term altogether and to simply substitute the Mendelian classification. When characters or traits occur in certain proportions under varying environmental conditions we may expect a given correlation with the elements of the germ plasm. A statistical report of the frequency of certain movements with respect to the chronological development of the organism is really what we are after. Whether we shall require ninety percent of 'presence' at an age of three months or only seventy-five percent before a given set of movements shall be called 'instincts' is really immaterial. When we actually have the statistical results, we shall find the term instinct superfluous. In the text I use the term in the rather loose sense as referring to the fact that in a large percentage of individuals,

4. The tonicity mechanism both local and general, through which the rate of the neuromuscular processes is regulated; *e.g.* the augmentation or depression of the rate of contraction or secretion in the muscles and glands as in excitement and depression.
5. The central or cerebral mechanism, through which receptor and effector functions interact with each other to modify the relations between stimulation and contraction; *e.g.* retention, learning.
6. The bodily response mechanism, made up of the sensorimotor connections and functions which represent movements, principally of the skeletal musculature through which the body assumes its temporal, spatial, and mechanical relations to the environment; *e.g.* walking, handling objects, the speech movements in language.

These various mechanisms are all interconnected with varying degrees of functional intimacy and the observable motor effect at any given moment is always a resultant of (*a*) the nature of the stimulating conditions; (*b*) the interactions between the simultaneous functioning of the mechanisms; and (*c*) the residual effects of past functioning on the present structure of the mechanisms.

Central or Cerebral Integration

The extreme variability of responses, the morphological complexity of the brain, and the obscurity which surrounds the actual processes of retention, learning, selection, has given to the cerebral mechanism a significance which, it seems to me, has hampered the development of a purely mechanistic explanation of human behavior. The non-scientific explanation of human conduct as the result of some special kind of force or entity such as psychical energy, consciousness, the ego, spirit, soul, cannot be successfully refuted if a description of the cerebral processes in any given response is demanded. The function of the brain in a response is an inference derived from a study of the *effector* conditions.

Suppose we consider the hypothetical case of an individual

certain relatively complex movements occur at an early age, and with practically no learning.

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whose contractile tissue is completely paralyzed but whose sense organs are functioning; whose neurons (afferent, central, efferent) are conducting nervous processes; whose glands are secreting; but in whom no muscle fibers are contracting. To what extent is this individual (who is of course absolutely motionless) responding to his environment? If the condition were permanent, an observer would be obliged to report the individual as virtually dead. The 'effectorless' individual himself might later report this condition as dreamless sleep or a period of oblivion. The question as to whether his subsequent behavior would be modified by the cerebral processes that occurred during the 'effectorless' interval I should be inclined to answer in the affirmative. That the amount of modification would be as great or of the same character as if the effectors had functioned, I should answer in the negative. I should regard the cerebral mechanism as one of the neuromuscular components contributing only a mechanical component to the variation in the behavior of the organism. As yet the relative resistance of neurons or synaptic connections and the effect of previous function on the cerebral mechanism is hypothetical, and this fact makes it practically impossible to meet arguments that are based upon some form of central adjustment or control in the organization of the movements that make up a response. Warren (**9**) for instance argues that, "the essential adjustment operations are central. Characteristic responses may be obtained *before* the implicit control responses are effective, in which case they would be determined solely by central conditions and by the nature of stimuli. If this be true, then the central process, not the response, is the significant feature of neural activity" (p. 267). And in conclusion, "If, as is here contended, the central portion of the nervous arc is most significant in the process" (the interaction between environment and organism), "then the fundamental concern of psychology is with the operations of the central nervous system" (p. 269). It seems to me that this excludes any recognition of what I have designated as behavior equivalences. If we ascribe them to the central nervous system, we assign to this structure a function (selec-

tion) that is non-biological. Is it not more scientific to assume that there are *no* central *control* or cerebral *integrating* mechanisms, and that the central nervous system is a system of conductors which seem to differ from metallic conductors of the type found in an electric wire network only in the property that the resistance of the synapses or neurons change in proportion to the amount of neural flux that passes over them, so that the distribution of the flux is constantly changing? The nature of the changes will depend primarily upon such factors as training, inheritance, and the immediate environment. Of course a certain normality of brain structure must be present, but even this is inferred from the behavior or the contractile effects rather than the other way around. While I do not wish to underestimate the importance of the cerebral mechanism, yet is not the stress we are now placing on it merely a disposition to place our problems in that pigeon hole which is least accessible? Are we not rather overawed by the complexity of brain morphology in assuming that brain function must be equally complex?

The Implicit Responses

When we consider the biophysical response and especially the internal motor patterns we find that the movements which an *observer* is able to record, represent only a small part of the movements which the *self-observer* can describe. The self-observer through the localization or description of the actual internal motor patterns may expand the response, so it will include older neuromuscular components which have disappeared and which now no longer occur under ordinary conditions. The systematic status of the implicit response lies intermediate between that of the biophysical response and the biosocial response. For the behaviorist the genesis of the implicit response, the 'image' and the 'stream of thought,' the introspective response, present his most difficult problems.

The Genesis of the Implicit Response.—Motor processes, especially those released by new stimulating conditions, are variable, not only with respect to the overt motor patterns

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but also with respect to the internal motor effects which are not readily localized or described except in their relation to the original stimulating conditions. The biophysical phase of the implicit response is a sensorimotor type of organization in which motor patterns that have functioned in the past may again function in the present without the original stimulating conditions, and at an intensity which cannot be discriminated by an outside observer.

To take a concrete case, let us consider the development of an implicit response while acquiring a new overt response, *e.g.* that of learning to write one's name in a new code form. At the first trial many more movements, internal as well as external occur than after the hundredth trial or after the action has been learned. If we enumerate the conditions that are operative at the first learning trial a list such as the following will result:

1. Some stimulus of the form to be copied; say a visual stimulus of the code form written on a piece of paper.
2. Accommodatory eye movements of fixating the stimulus, which alternate with fixations of the writing movements.
3. Organic conditions of perhaps strain and characteristic respiratory and vasomotor effects.
4. Tonicity variations in many of the postural patterns besides those that occur in the muscles of the writing hand.
5. Very complicated and relatively slow and unskilled movements in the muscles that move the pencil in copying.

During the learning period these conditions are constantly changing. After practice is complete and further writing shows no increase in speed or accuracy, all unnecessary movements have dropped out and those sensorimotor conditions that remain are the essential antecedents for the biosocial act of writing the code name. What are these essential antecedents?

(1) The original visual stimulus from which the name was copied is no longer necessary, since the code writing

may now follow upon an auditory and many other forms of stimulation.

(2) The accommodatory reactions of fixating the stimulus, and even those of fixating the actual writing, have become superfluous and the code name may be written with the eyes closed.

(3) The organic conditions are not the same. Just in what way they have changed cannot at present be determined.

(4) The tonicity of the muscles is differently distributed, but again a specific description of the changes is impossible.

(5) The actual writing movements are fewer in number, and better or at least differently coördinated.

We see that after the code-writing activity has been established the actual writing movements are the only ones that remain at all similar to the original learning movements and even here the similarity is not great.

Can the code-writing response be still further simplified? Would it be possible for instance, to leave off even the overt writing movements? This will depend upon the coöperative situation of which the code writing act has now become a part. If the situation actually requires the writing movements, no further reduction is possible and the code name is written whenever the *new* situation (not the original learning situation) occurs. However, the stimulating conditions may have so changed that even the writing movements are no longer necessary; as, for instance, when a rubber-stamp is substituted for the writing. The dropping off of the now auxiliary movements is not uniform and complete. The intensity of the various motor components gradually becomes less and any variations in the stimulating conditions may release now one, now another in the sporadic fashion in which our so-called imagery occurs. These residual auxiliary motor conditions are the components of the implicit response, which, of course, will be extremely variable on account of the many individual differences and combinations that are characteristic of the environmental and hereditary conditions under

which the individual has developed. An implicit response which may accompany any of the final code-writing actions, say a so-called visual image of the teacher who taught the code-writing, merely represents sensorimotor effect that has persisted long after most of the other secondary responses have disappeared. The conditions under which implicit factors may appear cannot be anticipated. They may occur at any time, even under conditions (dreams, illusions, etc.) that are entirely dissimilar from the original learning conditions. The neuromuscular phase of the code-writing, in changing to the rubber-stamp stage, has become a very significant coöperative response, that has no biophysical or neuromuscular relationship to the original code *learning*. No amount of the *reproduction* of any of the original neuromuscular conditions will reveal this biosocial character of the signaling or writing. Throughout life the individual is constantly acquiring new responses and old ones are disappearing, being modified, or being replaced. The residual effects do not disappear as rapidly as new responses are acquired. Our social responses change more rapidly than any others and during their formative period, which represents the period of youth in the individual, their implicit components are very numerous.

The implicit response *seems* often to be the essential antecedent of the overt response that follows. I may, for instance, in the act of mailing a letter, have a so-called image of a postage stamp. Shortly afterwards I actually affix a stamp to a letter. It *seems* as if the 'image' were the cause of the affixing act. But, stamps are affixed so frequently without preceding images, that their causal character is disproved. The image is rather the functioning of internal and rather obscure motor patterns which at one time were released by a visual postage stamp stimulus but which now are no longer antecedents in any sense of the stamp-affixing action.

I may describe my 'image' of the stamp as a 'red two-cent stamp' but this is actually a description of an earlier *stimulating* condition which released some overt response such as handling or inspecting a stamp. The actual *neuro-*

muscular conditions of the original response could probably not have been described even at the time it occurred. Whatever residual motor effects remain at this time are so obscure and weak that they do not even stimulate adjacent receptors and hence cannot be localized or described. The combined effect of the present stimulus and the residual effects remaining in the sensorimotor system may, however, release a verbal reaction such as 'I have an image of a stamp.' Even this response does not occur spontaneously for the average individual. The residual conditions must be reinforced by some such conventionalized auditory form of stimulation as: 'attend to your image,' 'report your mental states,' etc. The analysis of implicit reactions from the neurological standpoint would require a description of the exact sensorimotor conditions, but there are no precision instruments which measure the intensities of the neural processes in a neural network, or the weak contractile processes in the effectors, during the time the implicit response is actually occurring. The auxiliary motor processes or the receptors through which they were released cannot even be localized or designated at any time. As the interval between the original overt response and such residual effects as persist grows longer, the new environmental conditions which are gradually built up around the individual gradually obscure the implicit components so that they grow weaker, less detailed, and more irrelevant. Generally, as the individual grows older his implicit responses become less detailed and in some cases have been reported as practically vanished.

For the behaviorist the genesis of the implicit response lies in the auxiliary responses which occur during the learning of new overt responses, particularly those of the social or coöperative type. They represent residual effects, not causal elements in the formations of new responses.

The Image and the Stream of Thought.—The 'problem of the image' with its complications has always been regarded by those opposed to behaviorism as the stumbling block against which any strictly mechanical interpretation of human behavior must shatter. I shall try to show that the

mystery of the so-called imaginal processes is largely a language difficulty.

Consider the following concrete illustration: I am relatively inactive, sitting in a chair looking out of the window. One who observes me may not be able to detect in such movements as I make any relation to the surrounding stimulating conditions, nor can he anticipate the kind of a response I shall make to such a question as, 'What are you doing?' or more specifically, 'What are you thinking about?' Suppose I am asked the latter question and I report that I was just day-dreaming about a vacation spent in northern Michigan three years ago. Suppose I am then asked to be specific and describe the conditions. I begin by describing one of my images. "I had a visual image of a tent located on the shore of Lake Michigan. Its location was given by a break between two trees and an absence of underbrush, with a view of a sheet of water beyond. The image did not include the whole tent. There was a bare outline of the front with a partial view of the inside through the flap."

A behavioristic description of this 'image' would be as follows: I am able, even now, to describe (verbally) some of the original stimulating conditions of an event (the vacation) that occurred three years ago. The whole set of stimulating conditions at that time was strong. The sunset, dark woods, bright break in the trees, the silence and the contrasting white of the tent against the darker background, all of the stimuli released many sensorimotor, accommodatory, organic, and tonicity adjustments, with the external posture reactions which may be designated as 'admiring the scene.' The actual internal motor conditions were not localized at the time, nor can I designate them now with any degree of accuracy. Notwithstanding this fact I am able now to reproduce the implicit verbal response, 'What a beautiful landscape' which I made at the time. The total situation was such that all the sensorimotor processes that were taking place were modifying the neural interconnections between (*a*) the visual receptors that were being stimulated by the landscape and (*b*) the non-conventionalized motor patterns, such as

posture, respiration, circulation, tonicity, metabolism, etc. Furthermore, even though these were not localized or described by me at the time, the residual effects are still strong enough under the present 'article-writing conditions' to enable me to reproduce verbally some of the features of the landscape. I might even make a rough sketch of the tent that I have described verbally.

To do all this, implies (for the behaviorist) the functioning of implicit motor patterns and it is this obscure functioning which I would designate as 'the having of the image.' There is no *something* psychical or mental attached to or correlated with the motor processes. When I am asked to draw my image of the landscape, I produce such movements (drawing and painting) so that the observer's eyes are stimulated in the same way that mine were stimulated when viewing the landscape. In this there is the assumption that our sensorimotor systems are sufficiently similar so that when his eyes are stimulated as mine were stimulated, his motor response will be about the same as mine although I am unable to describe even my own effector pattern. Suppose I actually described or drew the imaginal neuromuscular conditions, as well as might be, and say to my friend, 'here is my image of the landscape.' I should not be meeting his implied request. When I am asked to draw my image this phrase biophysically means, produce in me a motor pattern which corresponds to the motor pattern which you had when your eyes were being stimulated by the landscape. Biosocially it means,—stimulate me in such a way that I will respond as you have responded. Obviously, a drawing of my neuromuscular pattern during the time I was having the image would not meet either one of these conditions. In fact, when I am asked to describe any event, this biosocially means,—stimulate me in such a way that I am able (without seeing the event) to respond as you have responded (after seeing the event). This may be done in two ways: (*a*) by stimulating another person's sense organs as mine were stimulated (by a photograph perhaps), or (*b*) by stimulating his ears with certain sound sequences (a verbal description). My verbal description is

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only a substitute form of biosocial stimulation which, within certain limits, produces in another the same neuromuscular conditions as the original stimulus produced in me. The fact that neither one of us is actually able to describe our own neuromuscular conditions does not enter in. It is not even probable that they are similar, especially when we consider, for instance, the sensorimotor systems of a young person of one social status and an old person of an entirely different social status. To stress the social or coöperative nature of imagery, we may translate the phrase, 'describe your image of the landscape,' into the biophysical and biosocial responses that are the equivalent of 'stimulate me in such a way that my sensorimotor organization will be so modified, that when I am asked to describe the landscape which you described to me, I shall be able to produce in a third person a motor pattern which resembled the one you had when your eyes were stimulated by the landscape.' It is in this sense that the image in spite of its apparent privacy is really a social phenomenon. It is a problem that arises when three or more persons form an interacting sensorimotor unit by interchangeably using each other's receptors and effectors. The so-called privacy of the image (or consciousness) arises out of the fact that the individual is unable to localize the anatomical and physiological structures that are involved. The processes of digestion, respiration, circulation, were just as private when the stomach, lungs, and heart were unknown. For an animal, for instance, all of the metabolic processes must be private occurrences, because it cannot describe them to other animals.

If I am not asked to describe my image, the neuromuscular conditions represent residual sensorimotor effects of earlier responses, that is all. What these neuromuscular conditions actually are has not yet been determined. When we know as much about sensorimotor function as we do about digestion, then digestion and imagination will be functions of the same order. That there seems to be a sense *quale* in addition to the sensorimotor conditions, is merely the fact that if my neuromuscular processes are actually photographed at the time

the landscape at which I am looking is photographed, even though the two photographs are not at all similar, it is still possible to work out a point for point correspondence or correlation between the two photographs and perhaps even classify the types of correlation into categories, the equivalent of what we traditionally call sense qualities.

In her presidential address Professor Washburn (11) maintains that the behaviorists have neglected the explanation of the sensation qualities by attributing them sometimes to the stimulus, sometimes to the response. Referring specifically to the sensation quality of blueness she states: "Obviously, however, the blueness is neither in the stimulus nor in the response, for these are movements and blueness is not a movement" (p. 92). While I would not undertake to prove that blueness is a movement until we have very carefully agreed upon what we are to include under the term movement, I believe that Dr. Washburn will grant the existence of a sensorimotor *correlate* of what she calls blueness. If it can be shown that this sensorimotor correlate is all that exists, then 'blueness' is at least a biological occurrence, and whether this is a movement or not becomes of secondary importance. As a behaviorist I would maintain that the correlate does not exist until the individual has learned to react discriminatively toward those spectral stimulating conditions which are verbally characterized as blue light by the physicist. To have the 'sensation of blueness' requires at least: (*a*) the spectral blue stimulus, (*b*) the specific retina elements, (*c*) a specific sensory-cerebro-motor organization which is the equivalent of the verbal sound-producing mechanism 'it is blue.' Now, is not 'the having the sensation of blueness' merely the actual occurrence of these three conditions? Blueness is not something in *addition* to these conditions. If the self-observer could designate all the anatomical and physiological factors that are involved, he would find merely a specific sensorimotor condition that is rather complex but always functions when acted upon by light of the specified frequency. Just what this response is, depends upon the biosocial conditions under which the individual has

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developed, but once established it is constant and this is all that is necessary. For the behaviorist the quality of blueness indicates the fact that the individual has developed a response that is specific for spectral blue, but that this response is implicit. The question, ' Is my blueness like your blueness?' is the same sort of a question as, ' Is my tallness like your tallness?' except in the latter case we can describe (measure and compare) the anatomical factors that are involved.

Any so-called stream of imagery or stream of thought is merely a complication of the conditions that have been described, and these complications reach their greatest complexity in those conditions characterized as thinking, creative imagination, reasoning, etc.

Professor Warren (10) has raised the question as to the behavioristic nature of 'thinking.' He states the problem in this form: "Is thinking essentially an *effector* phenomenon, or is it essentially a *neural* phenomenon? We do not know. The behaviorists believe that thinking is esssentially an incipient or implicit operation of the muscles or glands. The introspectionists believe it to be essentially an operation of the central nervous system" (p. 482). It seems to me that Warren has omitted what I have designated as the biosocial phase of the response. Biophysically, thinking is probably both central and effector, but this is true of all responses and if certain relations between sensory, central, and effector processes are of sufficient biosocial value to be grouped into a special category to which the name of thinking shall be given, our problem is that of designating what these relationships shall be, holding before us the historical and practical significance of this term. Proceeding along these lines most psychologists would agree that finding the square-root of a number includes what at least historically has been designated as thinking. Suppose I am asked to 'think out' the square-root of 144. I select this simple problem because the conditions for most of my readers will resemble my own. What has occurred in sensorimotor terms after I have 'thought out' the square-root of 144 as 12? I frankly agree with Dr. Warren that I do not know. There was certainly nothing so compli-

cated as the motor processes that occurred during the time in which I was learning to *recite* the square-roots of some of the easier numbers, or learning to *extract* the square roots of larger numbers. That there was any resemblance between the central neural processes that occurred during learning and those that have just occurred does not seem probable either. On the other hand would the solution of the problem in German be regarded as a different thought simply because the sensorimotor conditions were different? That there is something characteristic in the form of behavior which is called extracting the square-root of a number, seems certain. What are the specific factors? Without attempting an exhaustive analysis I find three relatively characteristic conditions: (*a*) a stimulus of the *problem* type, (*b*) an *interval* of longer or shorter duration, (*c*) a *solution* response. The traditional conception of thinking seems to imply that the thinker must be *able* to describe some earlier similar conditions (as imagery) during the *interval* but he must not *actually* describe them. The *solution* is an overt response which meets some biosocial condition that has already been standardized or that is on the way toward becoming a standardized response to this type of stimulus. The specific character of thinking may thus be regarded as the occurrence of an implicit response to a problem stimulus, in which the self-observer is able to (but does not) describe earlier but similar stimulating conditions, and responds overtly by some conventionalized response which is, or becomes, the solution.

I am not concerned whether my analysis of thinking into the three phases of problem stimulus, implicit response, overt solution resonse, is accepted as the *best* analysis. I wish merely to make the point that we are here dealing with complex biosocial responses for which there are no specific neural correlates. Any attempt to actually enumerate and isolate even the *effector* processes, say in my square-root illustration, is hopeless. To work toward an enumeration or description of the *central* neural processes as Warren seems to require, is certainly beyond any practical range. But suppose we could localize every effector that functioned and designate the in-

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tensity of its functioning; enumerate every neural element in the central nervous operation and its quantitative characteristics; could we from such data alone develop a classification which would conform to the historical meaning of the term thinking? Of course this is not essential; but then we may ask, why use the term at all? Are we not insisting that because faculty psychology created a faculty of thinking, there must be an equally specific sensorimotor correlate?

From the behaviorist point of view, the whole series of problems usually covered by 'the higher thought processes' reduce themselves to remnants of the same type as those which resulted from the investigation of the 'life principle.' Originally the word 'life' was begun with a capital letter and appeared principally in sermons. Now that life has been analyzed into complex metabolic processes we do not expect to find one super-physiological process that may be regarded as its essential characteristic. Just what the higher thought processes will become when reduced to human behavior I am loath to guess. I do not wish to imply that there may not be implicit sensorimotor processes and configurations that are simple and yet socially very significant, but it seems to me that it would be more expedient to begin with a genetic and biosocial study of the responses, and let our classification be derived from these investigations. When we shall have determined the biophysical and biosocial conditions under which such a response as that of 'acceleration' has been *acquired*, the problem of 'thinking' will be replaced by other classifications.

The Introspective Response.—In ordinary life the implicit motor patterns that accompany what we may designate as the *practical* act do not lead to separate designations or localizations. However, by such supplementary stimulation as the words, 'attend to your images,' or 'an introspective report will be required,' a given stimulating situation may release an altogether different series of responses than the same stimulus would have released under ordinary conditions. The auxiliary responses thus released originate partly from the residual and obscure internal motor processes remaining from earlier

responses and partly as a result of the instructions and the training in the introspective technique.

The introspective technique from the behaviorist's standpoint is essentially a conventionalized form of stimulation (instructions) which produces favorable conditions for the discrimination of (*a*) obscure motor effects, either simultaneous or successive, positive or negative, of the type known as sensations, after-sensations, two-point and difference limens, etc., (*b*) the residual effects of previous sensorimotor functions. After sufficient repetition and fragmentation, uniform descriptive reactions to these obscure sensory and motor conditions are established.

The extent to which these auxiliary types of reactions can be developed is well brought out in an article by Professor Wheeler (**14**). The problem was that of 'analyzing experience' under the following conditions. The figures 258 and 135 were arranged one over the other with a line under 135 as in ordinary addition and subtraction. The reagent (Wheeler himself) was instructed to *choose* between the alternatives of addition and subtraction in reacting to these numbers. Wheeler's report of his 'unanalyzed and analyzed experience' covers over a thousand words. Without the introspective technique and for the average individual the instructions to 'analyze your experience' would probably have released only the response, 'I do not know what you mean.' For the behaviorist, Wheeler's analysis is a specially learned form of behavior for which the stimuli are the obscure movements that act on the kinesthetic and other receptors adjacent to the muscle-twitches and other bodily processes that occurred during the experimental interval. It represents a delayed response, since the thousand words could not have been written or dictated while the so-called 'choosing' activity was in progress. As a problem in human behavior the experiment reduces itself to that of finding out what kind of an environment, training, and inheritance it is neceassry for an individual to have to enable him to accomplish what Wheeler has accomplished. That the introspection throws any light on the processes of 'choosing' is not clear to me at

all, if the final sentence, 'In none of these interpretations did the meanings mature until there developed strong motor tendencies to label them' (p. 434), is to be regarded as explaining the process of choosing. In any scientific sense the problem of 'choosing' can only be answered statistically—how many individuals will subtract, how many will add, under the given conditions. An *explanation* of the proportions can only be made on some sensorimotor or biosocial condition that in some way is different for those who *add* than it is for those who *subtract.*

If Wheeler's problem had been that of actually tracing out all the sensorimotor conditions that are operative and that can be established under the given stimulus situation, the introspective method revealed the existence of many neuromuscular conditions which could not have been isolated in any other way. But this makes the experiment one in the discrimination of obscure or residual motor effects, not one in analyzing the experience of 'choosing.' In ordinary scientific observation the responses of the individual to a given stimulus are classified with respect to the established *uniformities* of the various sciences (physical and chemical properties). In self-observation or introspection the responses are analyzed with respect to the *variety* of motor patterns that may be released. These are in any case practically countless and only limited by the response repertory of the subject. For the behaviorist the introspective reaction is a new technical overt reaction mechanism that is *added* to what might be regarded as the practical or socialized response to the stimulus and through which relatively obscure sensorimotor functions may be discriminated and localized. Professor Washburn (11) calls this 'symptomatic language behavior' and to me this seems an excellent characterization; but where does this leave the subject-matter of traditional psychology?

The Biosocial Responses (Overt Behavior)

By the term biosocial or overt response I shall designate those movements or motor effects which establish the individual's coöperative status in the social organization to which

he belongs. Speaking simply, the biosocial response is the group of movements or the effects of these movements that other individuals observe and classify as belonging to the educational, vocational, administrative, recreational, and personal forms of behavior. The systematic classification of these activities is properly the problem of the social sciences and behavioristic psychology participates in this problem by investigating the genesis and development of the responses as due to biosocial stimulating conditions acting upon specific types of sensorimotor organization. While socially the responses in promoting a new industrial enterprise show no obvious relationship to sailing a yacht, yet both forms of response may occur in the same sensorimotor system, and have many anatomico-physiological elements in common. The problem of behavioristic psychology is partly that of establishing the fundamental response categories that are operative in social organization. Concretely, the form of investigation which is known as mental, intelligence, educational, or personnel testing, represents the practical application of behavioristic principles, although the fundamental assumption (usually ignored) that this technique is measuring general mental faculties or functions, has restricted its usefulness as a scientific method. No attempt will be made in this paper to develop a systematic classification of the behavior categories, but I hope to indicate the lines along which they may be established.

The Recording Response.—This is a sensorimotor system usually, but not necessarily, of the speech muscles which upon functioning produce stimuli that act upon the receptors of other individuals in such a way that for individuals of the same social status these stimuli are the equivalent of, or a substitute for, the original stimulus which produced the recording response. It is thus essentially a conventionalized or socialized substitute stimulus for objects or events to which individuals whose sense organs were not stimulated by the objects or events might respond as if their sense organs had been so stimulated. Through the recording response the receptor-effector equipment of one individual is at any time

placed at the disposal of practically every individual of the same social status. Because of the permanent character in which it is expressed (writing, printing, etc.) it creates a secondary environment more stable than the primary enviroment of objects and events. Thus through a recording response such as language [1] an historical event which has occurred only once can be reproduced an unlimited number of times and practically at any time.

The Discriminative Response.—As a sensorimotor condition the discriminative response consists of (*a*) a specific stimulus pattern which acts on (*b*) a specific sensorimotor pattern which releases (*c*) contractions in some conventionalized effector pattern that is specific for the stimulus grouping. The act of discrimination is thus a *specific* response to a *specific* stimulus. In this category are included all those responses which are characterized as being qualitative or quantitative, *e.g.* red, bright, high, low, tones, odors, tastes, weak, strong, etc. The effectiveness of an individual in a social organization depends to a great extent upon the type of discriminations he has learned to make, and this indirectly determines his social status.

The Test for Discrimination.—The test as to whether a given discrimination is possible, is some conventionalized response. It is clear, however, that there may be a sensorimotor organization in which there is a specific response to a specific stimulus, but the specific response may not be in a conventionalized form. Under these conditions the individual himself would be able to discriminate, but he would be unable to enter into coöperative interaction with other individuals in whom the *conventionalized* expression of the discrimination is essential. The question arises, shall the discriminative response be defined in terms of (*a*) the sensorimotor mechanism, or in terms of (*b*) the conventionalized response? Unless the response, at some time, assumes a form that may be compared with conventionalized criteria for

[1] We are not concerned at this time, how these reactions are established, except that they are language responses. A detailed behavioristic account of the development of language and those categories that have been characterized as 'generalization and abstraction' can be found in Max Meyer's (6,7) books.

this particular type of discrimination, the existence of (*a*) the sensorimotor mechanism, and consequently the nature of the stimulus that released the response can never be demonstrated. Under these conditions shall we say that a stimulus does not exist? It seems to me that Warren's (10) thermometer fallacy rests upon this paradox. In his own words, "All the data of science are reported in speech or writing. It is a logical error (the 'thermometer fallacy') to identify the phenomenon observed with some secondary indication of the phenomenon. The observer's *report* is merely an indication of his *observation*. To assume that my experience of differentiating two colors is nothing but my verbal or written report of that experience, would establish a dangerous precedent. One might use it to prove that the tropisms of an amoeba which the behaviorist observes with a microscope are nothing but the behaviorist's own verbal responses or writing movements when he reports these observations. In suggesting this *reductio* I am challenging the logic, not the facts. For the locus of the observed 'experience' remains to be settled empirically" (p. 484).

The force of Warren's argument will depend upon how the terms observation and experience are defined. The physicist when he reports 'an observation' (a discrimination) implies the existence of some entity within him which 'observes' and the record which he (the physicist) makes represents a kind of verdict of this observing or discriminating entity. And to have an experience, assumes the existence of an 'experiencer' and an 'event' which is experienced. These are accepted scientific fictions recognized as figures of speech and their application in experimental methods has been so standardized and unified that the fact they are untenable theoretically may be ignored.

The psychologist, however, is the one who cannot accept this fiction without scrutiny and analysis. It is his business to ascertain just what the 'experience of differentiating two colors' actually means. Since science is perhaps one of the most highly socialized of the behavior categories it can only accept as a test for the existence of a discriminative response

some effector characteristic which places the response within the categories of science. The fact that it cannot be so placed does not demonstrate the absence of the stimulating conditions but it does remove them from scientific consideration. Even the term 'fact' which is one of the most firmly established fictions in science, is not a simple or clearly understood thing for the psychologist. While in general I am sympathetic with Professor Warren's 'reductio' yet I am only in the sense that if we use such terms as observation and experience we should define them in terms of our own fundamentals. For me, 'I am observing' only means 'I am responding'; and 'I am experiencing' also means 'I am responding.' When I am asked to define what I mean by response I can only refer to an analysis such as that which is the subject of this article, in which my fundamental assumption of an electron-proton movement continuum, defines the 'response' as one of the loci in this continuum. This meets Warren's requirement that 'the locus of the observed "experience" remains to be settled,' but not his requirements that it be settled empirically. I do not see how this could be done. I have adopted the method of mathematics, namely, that of *assuming* a locus for the response. Some assumptions are better than others. Mine has no special virtue except that others have already used it and the conception is easily understood. Unless, however, we agree upon what assumption we shall adopt, all controversy and discussion is futile, and no test for what we are to define as 'a discriminative response' is possible.

The Serial Response.—This sensorimotor type of organization is at the basis of the verbal more-less, greater-smaller, weaker-stronger, etc., forms of responding. It is the beginning of the series of responses (relations) for which there are (apparently) no stimuli. The problem is that of investigating how the individual learns and uses measurements in his coöperative activities.

The Relational Response.—In these responses the stimulus is a *relationship* (qualitative, spatial, temporal, social, etc.) between objects or events. The problem is that of determining how a *relation* between stimuli can itself become a stimulus

and release a specific response that is independent of the nature of the stimuli. In traditional psychology this was regarded as a mental process and when investigated by the method of introspection yielded auxiliary responses which were classified under various groupings of sensations, images, feeling, etc. The problem of determining how such a verbal response as 'one half' to two such different stimuli as, *e.g.* a half orange and a bottle of milk half full can develop, has always presupposed some non-biological factor (mind, meaning, consciousness) which did the relating. The relational response as formulated in its traditional paradoxical form 'if a relation between two objects is not a stimulus, how then can we react without being stimulated,' is answered behavioristically by the fact that our educational system is largely a device for supplying artificial objects (words, symbols, etc.) which become very definite stimuli for these relationships,

Generalization and Abstraction.—We have seen how, with the growth of complexity in sensorimotor organization, the language response becomes more and more important as a stimulus. The relationship is reciprocal. The growth of language develops more complex sensorimotor organization; and this in turn reacts upon the language responses until we reach a stage in which the behavior is apparently independent of the nature of the immediate objects and events that act upon the sensory mechanism. Thus a generalizing response such as the word 'food' is released by many forms of stimuli which are biophysically incommensurate, and on the other hand the word 'food' as a stimulus may release many responses (different ways of handling food). When this type of a sensorimotor organization includes *relationships* the responses are called abstractions. The stimuli are *relations* and the responses are of the recording type which for other individuals serve as substitute stimuli for the whole series of conditions under which the original relational sensorimotor patterns were established. The exact form which these generalizations and abstractions may take, depends not only on the sensory-cerebro-motor possibilities of the individual but upon the whole linguistic and social history of the race.

To quote from Professor Leonard Bloomfield's (1) recent work on comparative linguistics, "Chinese may, indeed, serve us as an example of a language with parts of speech entirely different from ours. It has no such parts of speech as noun, verb, adjective, and adverb. 'Good' is a quality, 'man' an object, 'speaks' an action in China as everywhere else, but the fact that these experiences belong to these different spheres is not expressed in the Chinese sentence" (p. 126). Facts such as these show just what traditional psychology has been designating as the *mental* factor. From the behavioristic standpoint the difference is only one of language habits and the oriental 'mind' is largely a linguistic difference as expressed in those responses which record the generalizations and abstractions of the social organization. For the behaviorist all this must be explained on the basis of what I have designated as the biosocial responses, and when the fiction of a 'mind' or 'psychic factor' is recognized as a pseudo-explanatory factor that has obscured the real nature of human behavior there should be less hesitation to perform the 'about face' in psychology which the situation demands, and which had its counterpart in biology when 'special creation' was replaced by evolution.

The Self.—The sensorimotor pattern which may be regarded as specific for this form of response is also a *relation* between stimuli. Mechanically we have two types of sensorimotor organization which may function with any form of stimulation. One type (*a*) made up mainly of sensory patterns which are only stimulated by conditions arising in what we designate as our own body and for which the conventionalized response is I, me, myself, etc.; another type (*b*) of those sensory patterns which are stimulated (relatively) independently of the body, and for which the conventionalized response is, you, it, they, etc. How this type of sensorimotor organization is established is unknown, but it should be clear that when we say a child has developed a consciousness of self or is self-conscious, this is no explanation but merely a very ambiguous and not a clear restatement of the fact that the child (or the reactor himself) has now developed a sen-

sorimotor type of organization that gives the 'I,' 'you,' type of response in the conventionalized manner. The essential fact is that a sensorimotor organization has been established which may function in such a way that only two specific types of responses are released, no matter what sensory patterns are stimulated.

For the behaviorist the concept of the 'self' is a specific response. It is not a non-biological entity which responds to the social and physical environment by having experiences, perceiving, feeling, thinking, etc., in the sense of the self-psychology, developed by Professor Calkins, which is dualistic, with the attributes or properties of the self (malice and envy, for example) being of essentially the same character as those usually described by traditional psychologists as mental. While there is, of course, no desire on my part to restrict or reinterpret self-psychology, I do not see the intimate relationship which Professor Calkins professes to see between self-psychology and at least my own conceptions of behaviorism. For me, the 'self' is one of a class of verbal responses which I make and use as a stimulus to act on the sensory mechanism of others under certain conditions, just as I use the words 'How are you' under certain other conditions. For the behaviorist who investigates the behavior which may be described under the traditional classification of 'the self' the study will be of the social or historical conditions which lead to the formation of this type of sensorimotor pattern. For this a study of comparative linguistic and experimental work in language formations will replace the usual introspective technique of traditional psychology.

Just as I have placed coöperative behavior at the basis of social organization, so some of the coöperative responses may enter into reciprocal relations with other of the individual's own movements. This may be called self-activity, purpose, etc. Just as the individual is a coöperative unit in a social group so his own action at any given moment may be regarded as a coöperative unit in his own life history.

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Language Behavior

I have stressed so persistently the language response as an important factor in those responses which I have characterized as discriminative, relational, generalization and abstraction, that we may ask, is language necessary for the development of these sensorimotor types of organization? Do not animals, who have no language, make generalizations? Suppose for instance that in place of the word 'enough' an individual had developed the specific but unconventionalized response of folding the arms. Under these conditions there would be no way of demonstrating that the individual had developed the generalization until some one had observed that folding the arms was the equivalent for the conventionalized 'enough.' Suppose further that instead of folding the arms the specific effector pattern was internal and so obscure that no external observation would reveal it. For the individual working by himself only the *specificity* of the motor pattern is significant; he would carry a task to the same degree of completion as others and could work in coöperation with them, although other individuals would be puzzled to explain this coöperation. The traditional psychologist would explain the fact by saying that the individual had the *idea* of when a task was to be regarded as completed without an adequate motor expression to indicate to others that he *understood* the situation. The behaviorist would explain the fact by saying that the individual had acquired the proper habits, but not the conventionalized *name* for these habits. That is to say, the functioning of the arm muscles in folding was not a motor pattern which *for other individuals* acted as a stimulus to establish the fact that this individual could act as a coöperative unit in the performance of a series of coöperative activities, and it is certain that he could never *direct* these activities.

From the biological or behavioristic standpoint the question resolves itself into one of: How important is the conventionalized response for survival? In the evolutionary series there were probably animal forms whose sensory acuity and bodily strength exceeded that of man's progenitors. But as soon as a response becomes socialized it multiplies the proba-

bility of survival for the group even though the socialization develops subsequently into injurious superstitions and taboos that may actually diminish the individual's acuity for developing useful generalizations.

It appears that whether a response is or is not socialized is significant because this determines the effectiveness with which the individual can enter into coöperative interaction with many individuals. In animals living in herds, or during migrations, or in their family life, some generalizations seem to have been developed. In man, language, because of its permanence as a response and independence of the nature of the objects or events for which it is a substitute stimulus (its symbolic character), has made possible the development of generalizations and abstractions which far exceed those of any other animal. It is through the specificity of the response to complex environmental conditions and relations that the individual's effectiveness as a coöperative unit in a social organization is determined and unless these specific motor patterns also act as specific stimuli on others they do not meet the coöperative requirements. Now it is precisely the conventionalized or socialized phase of the discriminative, relational, comparative, etc., form of sensorimotor organizations which form, as it were, an index of the individual's effectiveness, and this index is what we mean by language. For the behaviorist human achievement as represented by the educational, vocational, administrative, recreational, and personal activities of individuals is the result of sensorimotor mechanisms which belong to various categories of responses which genetically trace back through hundreds of individuals and through environmental conditions which now no longer occur. This only means that in human behavior we have developed a new type of sensorimotor organization, the *recording response* in addition to the discriminative, relational, and comparative, type of sensorimotor organization. The recording response (as language) makes it possible for one individual to act as an intermediate stimulus-response unit in a coöperative response series (industrial relations, the labor movement, democracy, prohibition, etc.) involving hundreds of individuals and unlimited by time and space.

The Aims of Behaviorism

The most obvious statement in regard to this question is that they have not as yet been clearly formulated. On the negative side we are fairly well agreed that traditional psychology in its many forms does not promise a scientific solution for the explanation of human conduct. This problem arose when the theory of special creation was challenged by Darwin's theory of organic evolution. If species were not specially created, perhaps human conduct or human behavior was also merely a development of the biological and physical conditions which were in operation as far back as this planet was fitted for life. Just as we have the problem, how did species arise, we have coupled with it the problem of how human conduct arose. Is there, then, a problem of social evolution as there is one of organic evolution, and is the sensorimotor system of the individual the sole datum in social evolution? Behaviorism is trying to give the answer in the same terms that have made biology into biochemistry; have made chemistry into physical chemistry; have made physics into mechanics and mathematics. Just as organic evolution repudiated the existence of a supernatural or non-biological force which kept species immutable, so behaviorism in the explanation of human conduct repudiates a supernatural or non-biological force which keeps human behavior immutable, or to be more specific, which keeps human behavior controlled by teleological or vitalistic principles beyond the scope of the mechanistic explanations that have been found more and more adequate as our researches into physical phenomena become more thorough.

Even traditional psychology is tending in this direction. It is expressing a general regret that such terms as mind, mental, consciousness must still be used, but consoles itself partly with the belief that since everyone knows what they mean, a definition is unnecessary. To fill up the gap left by this enforced self-restraint in the use of psychical causal elements, there is however a reckless abandon in explaining human achievement as due to 'drives,' 'meanings,' 'instincts,' 'desires,' 'psychophysical tendencies,' etc. The subject

matter of traditional psychology is changing so rapidly that the newer texts have little more than the word psychology in common. I suppose one ought to be charitable and regard this as a 'healthy sign' and that out of the Psychology of This, That, and The Other Thing, the '*True Psychology*' will be born. Of course, all of these books claim to do justice to behaviorism. Even for those who sincerely try to understand behaviorism, the social traditions often prove too strong for clear perspective. Instead of man being the center of all things and civilization a device through which he demonstrates his superiority or inferiority to some mystical super-power, behaviorism places man at the end of a series of electron-proton organizations of which his civilization is only a device through which greater aggregates and more complex electron-proton organizations become possible through the coöperative elimination of the restrictions imposed by disease and death, restricted sensory range and environment, the restrictions of faulty inheritance, the restrictions imposed by the struggle for food and shelter, the wasted energy in the competition between individuals. The behaviorist problem is that of studying the processes through which man as a biological unit approaches a condition which may be described as omnipresent, omniscient, omnipotent, not in the magic meaning of these terms but as one limit in cosmic evolution. The social organization known as The State realizes these conditions more fully than any individual in it, but to the extent that these conditions are realized by social organizations, the individuals in them will also achieve them.

For me this is no hallowed destiny. It is derived from the electron-proton conception, and man's place in the system is as much a matter of mechanics as the rainfall and the tides. For me social evolution is a more complex form of organic evolution, but never anything more than problems in mathematics, physics, chemistry. Do our modern text-books on physics contain the type of equations that express those electron-proton relationships which are most useful in describing the physical principles underlying the social phenomenon of say '*democracy*'? I do not know, but I think such

equations can be and will be discovered, and as a behaviorist I regard it as my privilege to contribute toward such investigations. If human behavior differs from physical and chemical behavior, it is only in the fact that the parallelogram of forces that are operative in determining the energy and direction of human movements, contain components which derive their magnitude from human movements that are outside the sensory range of the individual whose responses they actually modify. In other words, the *recording* response as exhibited in language makes it possible for past environments, the responses of individuals that are distant with respect to time and place, to function as forces which may be more effective than the unmediated internal and external bodily conditions at any given instant.

My behavioristic colleagues with the exception of Max F. Meyer, my teacher, will criticize my introduction of the cooperative element in human conduct because they will see the danger of its being regarded as a terminological variant of mind, consciousness, etc. I have tried to show that this is not the case. Next to sociology I find more worn-out psychology in physiology and neurology than in any other science. To hop over these sciences and jump directly into physics and mathematics seems to me the more effective step in the long run. While I believe I appreciate as fully as any one else the need for sensory-cerebro-motor research, I think this is so generally recognized that no special emphasis is needed. More pressing, it seems to me, is the need for appreciating that human behavior is an evolutionary device through which a new form of physico-chemical combination (the *compound* multicellular form of organization) is being established as a further development of organic evolution. It has taken the biological sciences fifty years to eliminate 'vitalism' and get onto a physico-chemical basis, and it does seem as if behavioristic psychology should be able to eliminate psychological 'vitalism' (mind, drives, psychophysical predispositions) and introduce the mechanistic conception of human achievement and the evolutionary character of social organization, in a shorter time and without repeating the behavior correlate

of the Darwin-anti-Darwin controversy. Only time will show whether any considerable amount of 'short-circuiting' will have taken place.

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SELECTION IN 'TRIAL AND ERROR' LEARNING

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McDougall's recent challenge[1] to the comparative psychologist to explain the shortening of the trial and error series in terms of mechanical bodily movements makes it highly desirable that just such an explanation be presented. An adequate explanation of the process of selection of overproduced movements and their arrangement into a final perfected habit is fundamental for psychology. Until the agents of selection are known the origin of such skilled acts as speaking, writing, playing musical instruments, laying bricks, playing tennis, manipulating a knife and fork, and the like, will remain a mystery. Since the classical experiments of Thorndike in 1898, hundreds of cats, and rats by the tens of thousands, have been studied painstakingly in maze and puzzle box without any appreciable increase in our understanding of the processes observed in the 'trial and error' process. The empirical facts could not be more obvious, nor the theoretical explanations much more inadequate or contradictory. It is the purpose of this paper to show wherein the most widely accepted theories of selection are inadequate and to propose an explanation (not original with the present writer) which appears to be entirely adequate.

For the sake of clearness four principal problems of selection may be recognized. To be complete (1) any theory of selection must explain the origin of the varied reactions of the animal in response to the total situation which confronts it; (2) the nature of the process by which certain variants are selected and appear in the final habit; (3) the nature of the process by which certain other variants are eliminated; and (4) the nature of the process by which the selected variants

[1] McDougall, W., 'Outline of Psychology,' p. 186.

are given with increasing facility and resistance to distraction. Naturally the problem is not exhausted by such an enumeration. However, the minor problems can be solved more readily after these principal difficulties are cleared up.

Since the earlier statements of selective agencies in learning (going back to Spencer, Bain, and Maynert) were based upon general observation rather than upon experimental studies and are in every way inconsistent with modern neurology, they demand no extended examination. The same may be said of the various teleological explanations in which selection is made to depend upon the purposive nature of mental activity. The scientist prefers to bind himself to the common sense assumption that needing a piece of meat does not open the door of the puzzle box for the kitten. Nor will consideration be given to those theories which assume that the success of an act (from the point of view of an outside observer) strengthens the selection of this act. The situation is not so desperate that we must yield to these too easy verbal solutions. They will always be available in case a vigorous attempt at scientific description fails.

The solution proposed by Thorndike has had the greatest influence upon other writers. Thorndike has stated that all selective learning can be explained by a law of effect and a law of exercise. The importance of these laws justifies their quotation in the words of the author.

"The law of effect is that: Of several responses made to the same situation, those which are accompanied or closely followed by satisfaction to the animal, will, other things being equal, be more firmly connected with the situation, so that when it recurs, they will be more likely to recur; those which are accompanied or closely followed by discomfort to the animal will, other things being equal, have their connections with that situation weakened, so that when it recurs, they will be less likely to occur. The greater the satisfaction or discomfort, the greater the strengthening or weakening of the bond."

"The law of exercise is that: Any response to a situation will, other things being equal, be more strongly connected

with the situation in proportion to the number of times it has been connected with the situation and to the average vigor and duration of the connections."[1]

The law of effect, understood as Thorndike interprets it, is free from the vulgar errors of the pain-pleasure school of interactionists. Its difficulty is that it leaves a solution of the problem as far away as ever. When placed in the puzzle box the cat, to situation 1, makes response *A*. This response changes the external and internal situation of the animal, bringing response *B*, which is followed by situation 3 bringing response *C* which causes a 'satisfying' state of affairs to exist for the animal. After a sufficient number of trials, situation 1 brings response *C*, the intervening reactions not appearing. The law of effect is an interesting generalization but it leaves the question: Why, from the point of view of the animal's structure, do those acts that are accompanied or closely followed by 'satisfaction' tend to come earlier and earlier in the trial and error series?

The author of the theory offers various tentative hypotheses of the nature of the neural changes involved in learning under the law of effect. These hypotheses are concerned with effects upon the neurones or their synapses brought about by conduction of impulses from receptors, response to whose stimulation brings satisfaction or annoyance. "A neurone modifies the intimacy of its synapses so as to keep intimate those by whose intimacy its other life processes are favored and to weaken the intimacy of those whereby its other life processes are hindered."[2] There is not a shred of neurological evidence for this assumption. In the first place it is highly probable that all neural impulses are of the same quality and more than likely that all are of the same intensity. In the second place the response threshold to an 'annoyer' may show either positive or negative adaptation, depending upon various factors in the method of presentation. The sour grape fruit and the bitter beer are 'annoyers' of the young child. In later life these substances may be prized for the

[1] Thorndike, E. L., 'Animal Intelligence,' p. 227.
[2] *Ibid.*, p. 247.

very qualities that once conditioned their rejection or by appropriate training the original avoidance reactions to them may be intensified. In neither case is it possible to correlate these modified reactions to the life processes of the neurones in any such way as the law of effect demands. The law of effect with its neurological implications is criticized in detail by Holmes, whose suggestions have received less consideration than they deserve.

The second of Thorndike's laws of learning, the law of exercise, is not concerned with the agents of selection, but with the working of the selected mechanism. Taken by itself it would explain the fixing of the varied reactions in their original form and thus tend to counteract whatever selective factors operate. 'Other things being equal' the law of exercise effectually prevents the shortening of the series. The laws of exercise and effect must definitely be abandoned.

This is recognized by Watson in his 'Behavior.'[1] Disclaiming any intention of offering new principles of selection, Watson insists that two factors, frequency and recency, are sufficient to account for the facts. In a later work, 'Psychology from the Standpoint of a Behaviorist,' these factors with two additional ones are offered as 'some attempted causal explanations of fixation' and it is emphasized that they are little more than speculations. In general, Watson's readers have ignored the warning of the later publication and are inclined to accept the earlier solution as satisfactory and complete. It can be shown that neither of these explanations is helpful.

The principle of selection is developed by reference to certain laws of probability. The argument is that " . . . we know from the general theory of probability that in any chance temporal arrangement of events that the probabilities are equal, that any one of them will precede or will follow any other, and that in a large number of such chance arrangements of the same event any given event will precede or will follow any other, and that in a large number of such chance arrangements of the same events any given event will precede each

[1] Watson, J. B., 'Behavior, an Introduction to Comparative Psychology,' p. 256.

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of the others in just half of the arrangements and will follow in the other half. But, if now each temporal series is *interrupted* at the occurrence of the given event and the occurrence of all that follow is prevented, the *given event* will occur twice as frequently as any other. A concrete example will illustrate this point. If ten slips of paper, numbered consecutively from 1 to 10 are drawn at random from a box until a certain number, say 5, is obtained, the chances are equal that the required number 5 will precede or follow any other number, 9 *e.g.* If the drawing is interrupted when 5 is obtained, and all the slips are returned to the box, and this procedure is repeated a number of times, 5 will be present in every series of drawings, the number 9 in only those in which it, by chance, was drawn out before 5. Since this condition is met by only one half the series of drawings the number 9 will appear only half as often as any other." One assumption is held necessary to complete the analogy: "For the operation of this principle of frequency it is necessary that the activities set off by the situation in the puzzle box be diverse—that the animal go to some new activity rather than continue to react in a circular way—*e.g.* to spend all his time biting the wire."[1]

The fallacy of the frequency argument in a somewhat similar form was pointed out by Thorndike in 1911. McDougall has shown its fallacy when applied to maze learning. The error lies in a gross misuse of the argument from analogy. There is no essential similarity between the behaviour of the kitten in the puzzle box and the drawing of slips of paper from a box under the conditions described. The theory demands that no circular activity occur. Circular activity, however, is something that every investigator has noted although, so far as the writer is aware, its amount has never been measured by counting the number of times the animal claws at the various bars, attempts to push between them, rubs against them, and the like, before the consummatory response is given. Thorndike describes typical behavior when he says, the cat "would show evident signs of discomfort and of an impulse to escape confinement. It tries to squeeze through

[1] Watson, J. B., 'Behavior, an Introduction to Comparative Psychology,' p. 263.

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any opening, it claws and bites at the bars and wire, it thrusts its claws out through any opening and claws at anything it reaches, it continues its efforts when it strikes anything loose and shaky, it may claw at things within the box. It does not pay much attention to the food outside, but seems simply to strive instinctively to escape confinement. The vigor with which it struggles is extraordinary. For eight or ten minutes it will claw and bite and squeeze incessantly."[1]

If each repetition of an act increases the probability of its repetition on a succeeding trial one would expect that several minutes of vigorous clawing, biting, and pushing would tend to fix these mechanisms rather than that involved in the successful response that comes but once each trial. Instead of this, if the animal succeeds finally the very movements most practiced in the earlier trials must be abandoned. Also, far from being 'infinitely varied' the responses of the animal are strikingly stereotyped, clawing, biting, pushing on the same bars many times repeated. Nor is the final successful response always given in the same way, as is assumed. At one time a clawing movement of the right foot leads to freedom, at another an upward movement of the nose, at another a striking movement with the left foot, etc. Finally, when an animal has been put through a series of puzzle boxes of somewhat similar construction but demanding quite different successful responses, its tendency to bite the bars, push against them, and to claw at holes is weakened, although bars and openings are the feature common to all the situations and the responses to them are the most practiced.

The analogy suggested by Watson needs modification in some such way as follows: Since the successful response comes late in the first trials (there would be no puzzle box situation if this were not the case) and since it is preceded by a vigorous and repetitious series of other movements, the slips bearing the numbers of the successful reactions should not be placed in the box until a large number of other slips have been drawn out. Furthermore, since many unsuccessful reactions are repeated many times, the larger number of slips should be

[1] Thorndike, E. L., 'Animal Intelligence,' p. 35.

returned to the box as soon as they are drawn out. Since by hypothesis, drawing the number of a successful reaction ends the trial, this response will come but once in each trial, and hence will be the least practiced.

McDougall has pointed out a similar fallacy in the use of the frequency principle as a selective agent in maze learning. Given a simple maze in the form of the letter Y, in which the animal is started from the base and in which the left arm is a blind alley and the right leads to food, and chance being the only selective factor, in N trials each alley will be selected $N/2$ times. Since selection of the right alley ends the trial, one half the trials will have this response. If the left alley is entered first, the animal returns eventually to the starting point, where by hypothesis the probabilities of selection are again equal. Watson diagrams a situation in which the orientation of the animal in leaving the blind alley will carry it into the correct pathway. Obviously this maintenance of orientation will increase the number of entrances of the correct alley only when the blind alley is a continuation of the pathway leading to food. In the Y-shaped maze this same maintenance of orientation will carry the animal back to the starting point and the frequency argument breaks down.

A fallacy of a somewhat different sort lurks in the application of the frequency principle to a somewhat more complicated maze. If a maze in the form of the letter T be taken, in which the animal is released at the bottom and the correct response is a turn to the right, the probability that the animal will make the wrong turn to the left and returning again select the wrong passageway, thus going to the point of release, is $\frac{1}{4}$. The corresponding probability that the correct response will be made is $\frac{3}{4}$. True as this is, it explains nothing more than why the rat will get into the right pathway rather than back to the starting point, in 3 trials out of 4. It throws no light on the 100 percent of errorless trials that eventually occur. The implicit assumption here seems to be that the training effect of walking straight ahead out of the blind alley into the proper pathway will make, on the following trial, a turn to the right at the top of the maze more

likely to occur. There is no evidence for such transfer of training among rats.

Furthermore, the principle of frequency does not explain why the culs de sac after sufficient training not only fail to arouse the entering reaction as at first, but arouse a positive avoidance reaction, so that the correct pathway being barred, the animal will fail to enter the cul de sac until driven by unusual hunger or discomfort.

The second principle, that of recency, is difficult to apply. Watson, who proposes it, asserts that it is much less potent in habit formation than frequency and adds that in certain habits, such as maze learning, its influence cannot very well be made out. Carr, who has also proposed it as a factor, has stated the strongest objections to its use. Until the efficacy of recency can be shown it should not be considered as a selective factor.

The way is now clear for a type of explanation that makes use of none of the erroneous assumptions concerning the efficacy of satisfaction, probability, or recency of the current theories. In its original and incomplete form this explanation is older than the conventional theories, and its development has been parallel to them. In 1896 Lloyd Morgan sets the problem of why the second cinnabar caterpillar given the chick brings the avoiding reaction instead of the pecking response, and discusses the way in which 'the disturbance associated with taste is directly called forth by the disturbances associated with sight.' In more modern language, the reaction given at first to the caterpillar as a gustatory stimulus is now given to it as a visual stimulus. Hobhouse (1901) diagrams the reactions and explains the rejection eventually elicited by the sight of the caterpillar as due to arousal by the taste of the caterpillar of an inhibitory wave overlapping the original excitement and modifying or cancelling it. Holmes[1] offers an extension of the explanation of Hobhouse. These discussions, particularly that of Holmes, deserve more consideration than has been given them. If this had been done at the beginning of the century, twenty years of work in a blind alley would have been saved.

[1] Holmes, S. J., 'Studies in Animal Behavior,' Ch. 7.

Two recent writers[2] have put forward an account of learning which continues the development of the work of Morgan, Hobhouse, and Holmes, with additions that provide a complete and satisfactory explanation of the process. Their explanation will be taken up under the headings of the four chief problems of selection. As a specific example, the classical behavior of the cat in the puzzle box will be taken.

First, as regards the origin of the useless movements: "The cat when in confinement is instinctively organized to respond to the sight of bars, cracks, corners, and even solid walls by approaching and pulling and pushing with paws, claws, nose, and teeth. If any part of the box is loose, the cat has a tendency to continue its manipulation. He is also organized to turn away from these same objects when they offer more than a certain amount of resistance to his attack." The behavior of the cat is definitely determined by the structure of its nervous system and the various stimuli which act upon it, with minor fluctuations due to hunger or fatigue.

The elimination of certain of these 'random' movements and the retention of others is explained through conditioning. "A *single object* may be the source of stimuli first to distance receptors such as eyes, ears, or nose, and later to the sense organs in the skin, muscles, and tendons. . . . There is a classical example of the baby and the candle. The baby *seeing* the flame holds out his finger, and on *feeling* the flame withdraws his finger. [In this case] . . . "*while the approach stimulus is still present* the avoidance reaction is called forth. *The result is that the avoidance reaction is now conditioned on the approach stimulus so that the avoidance reaction tends to be given as soon as the approach stimulus is received.* The sight of the flame has become the conditioning stimulus for withdrawing the finger." The same process occurs in the puzzle box illustration. "The situations which the cat faces in the puzzle box are for the most part composed of visual stimuli which attract him, followed by stimuli to his proprioceptors and sense organs of touch which repel him, and these two

[2] Smith, S. and Gutherie, E. R., 'General Psychology in Terms of Behavior,' p. 119–130.

classes of stimuli *are given by the same object.* He is instinctively attracted by the sight of the bars, but on reaching them, especially if they are rigid, he is instinctively impelled to turn away. This turning away is due to the resistance of the bars to his attempted manipulation and to his efforts to force his way between them. Approach and avoidance are here original tendencies called forth by the same object. With repetition the sight of the bars becomes the conditioning stimulus for retreat, so that the conditioned response inhibits the original response. One by one the movements of approach to the various confining surfaces of the box are inhibited by the conditioned responses of retreating, until at last the animal is attracted by the door-opening device. The reason that this last movement is not inhibited is that the device itself never serves as the source of a stimulation which is instinctively avoided. Although the cat turns away from the button in response to the open door, he does so not because the button repels him, but because the open door attracts him. Approaching the button and approaching the open door are the only approach responses which are not inhibited by conditioned avoidance responses, and while the door is closed the button alone calls forth an uninhibited response." Eventually repetition establishes these conditioned responses so that, provided the avoidance stimuli are sufficiently strong, the approach responses are no longer given. In the rivalry of approach and avoidance the emotional reënforcement that is usually associated instinctively with avoidance gives these movements an advantage that hastens the inhibition of movements of approach.

Since all responses do not bring avoidance reactions into play fruitless movements may be retained in the final resulting habit. A persistent tendency to rattle a loose bar may often be observed. Responses to the loose bar fail to bring into play the avoidance reactions that are given to a rigid bar, and so are retained. The present-day efficiency expert exists in part to provide avoidance stimuli which eliminate superfluous movements fixed by reason of the absence of innate avoidance stimuli when the worker began the trial and error learning of his trade.

This process of conditioning is not the only one involved in the shortening of the 'trial and error' series. "When a number of acts form a serial response, one act is seldom completed before the next is begun. In this way there is an *overlapping of compatible movements*, . . . This overlapping is made possible by the fact that many conditioning stimuli have been acting for some time before the response which they have come to condition has occurred." Jennings in 1906 attempted to explain overlapping of this kind by the formulation: "The resolution of one physiological state into another becomes easier and more rapid after it has taken place a number of times." Like the law of effect this is a generalization rather than an explanation and it is impossible to state it in neurological language. The foregoing explanation in terms of the overlapping of compatible movements is open to neither of these objections.

The explanation of the origin and principles of selection of movements in 'trial and error' learning sketched in this paper has been conducted entirely in terms of familiar processes. It rests upon no analogy and makes no assumptions which the neurologist is not willing to grant. To the present writer it has seemed to cover adequately every case of 'trial and error' learning to which it has been applied.

A PSYCHOLOGICAL DESCRIPTION OF INTELLIGENCE

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The purpose of this paper is to present a fairly detailed psychological description of what we ordinarily mean by intelligence. Obviously such a description will include a definition of intelligence and before defining it one should have his point of view clearly in mind. For instance is the definition to be a utilitarian one? Is it to be biological and expressed in terms of adaptation of the individual to his physical and social environment? Is it to be technically psychological and if so from what point of view? Accordingly the definition of intelligence which is to follow aims to be technical, behavioristic and in a way couched in terms of adaptation.

Furthermore, a definition of intelligence is bound to be vague, general, and superficial if expressed merely in terms of capacities or abilities. It ought, rather, to take into account those qualitative details of behavior which we call judging, reasoning, comprehending and the like. What we mean by these terms should be stated very explicitly. Any definition of intelligence is incomplete, then, which does not rely to some extent upon data of introspection. It is futile, also, to attempt a definition of intelligence in terms of such meaningless artefacts as 'specific' and 'general' factors. Such data on intelligence as are yielded merely from statistical methods will never furnish adequate concepts. Such attempts lack the necessary qualitative information or else this information is inferred from a very general and superficial knowledge of human nature. Indeed, what is the discussion of general versus specific factors all about, anyway, when a general factor does not function except in specific ways and when a specific factor becomes generalized only through the mechanism of abstraction?

Likewise to define intelligence in terms of its supposed components such as judgment, abstract thinking and the like, does not tell us what intelligence actually is any more than to assert that a chair is made of wood defines a chair. Intelligence is something more than the sum of all its parts. It is action as well as content.

The following description of intelligence is based upon the evident fact that the rôle of consciousness in life is two-fold: (1) it directs or coördinates overt behavior; (2) it produces adaptive movements in times of temporary crises in behavior when instinct and habit are for the moment inadequate with respect to the organism's needs. Were it not for this second rôle of consciousness, action in times of crises would be confined to a trial and error procedure. Consciousness, without the development of explicit meanings, functions in the first rôle; and consciousness with the development of explicit meanings functions in the second rôle.

For practical descriptive purposes, bearing in mind that any classification must be more or less arbitrary, we shall give to these modes of functioning of consciousness the name 'covert behavior,' and we shall classify each according to its four outstanding levels of complexity. The primary and simplest level may be called the sensorimotor level; the next may be called the perceptual-motor level; the next a judgment-motor level and the fourth and most complex, the reasoning-motor level.

Presumably the simplest response which a human being makes to his environment and to his own bodily conditions (excluding reflex action) is sensorimotor. The sensory or covert stage of this response is at first vague and indefinite and the corresponding overt stage is a poorly coördinated movement which we may call an impulse. The antecedent or stimulus of an impulse, then, is sensation. This sensory experience lacks spatial projection and localization at the outset. Gradually, however, as a result of overt reactions to environmental stimuli the individual becomes oriented in a world of three dimensions. Delicate and incipient motor adjustments, including convergence and accommodation, be-

come integrated with these sensation-responses. As a result of this there develops a secondary type of sensorimotor reaction which we shall call 'derived covert behavior.' This is the phenomenon of meaning in its simplest form, and this meaning is projection or localization—the hereness or thereness of objects. It is simple perception.

At the same time, in covert behavior, there is differentiated from sensory response another type of sensorimotor reaction, namely the image-motor reaction. This latter becomes integrated with the derived covert reaction already mentioned, making the perception more complex. Now the individual not only localizes an object in space but tends to label it with respect to previous experiences. As language responses develop he goes farther and tends to give an object its name as a part of his perceptual-motor reaction. By this time perception approaches what we ordinarily call the process of recognizing. It is now an object, to which the individual responds, and it is a particular object as well as an object localized in space.

This primary, derived covert reaction enables the individual to make more useful and adaptive movements, and movements previously made have assisted in the development of the derived covert reaction through proprioceptive stimulation. Now the individual reaches for and handles objects less awkwardly; his movements are more direct, rapid and better organized. His newly acquired derived covert behavior plays an important rôle in definitizing his gross movements. Eventually, then, perception becomes the covert stage of habitual actions and of modified or adapted instinctive responses.

No sooner do perceptions develop, however, when they in turn become the 'cues' for types of motor response other than gross movement or motor habit. They lead as well to incipient motor attitudes, gestures, and language responses by means of which an object is responded to as familiar. An object is identified, or it is explicitly referred to some experience in the past. In the less attenuated forms of recognizing, imagery of some sort plays an important rôle

as a cue for such incipient motor adjustments as are directly involved in identifying the object in question. Thus we have, in recognizing, a slightly more complex and more explicit development of meaning than in perception. It is another form of derived covert behavior. It is, again, a simple process of knowing and the one factor, above all others, which makes it a process of knowing, is incipient motor adjustment—a facial expression, a motor attitude, a gesture, a language response, or several of these combined. Just as perception is a form of knowing what we sense, recognizing is a form of knowing what we perceive.

Just as perception redirects the cruder sensorimotor reaction and renders it more adaptive, recognition organizes and redirects our perceptual-motor responses. We do not ordinarily recognize unless there develops some temporary crisis or implied problem in our instinctive and habitual behavior. For example, the usual procedure in approaching a door is to perceive the knob and to make the motions of reaching and turning. Let it be a peculiarly shaped knob, however, or let someone distract our attention momentarily just as we are about to pass through the door, and our perception becomes elaborated to the stage of explicit recognizing. There is implied the problem: "Is this thing the knob?" "Is this the thing you turn?" "Where is the knob?" The crisis need not be realized as such; it need be present only in a momentary blocking of one's habitual reactions to doors and knobs or it may consist only of a slight hesitation, with a developing attitude of surprise or uncertainty. Under such circumstances recognition furnishes the necessary additional cues by which a confused set of movements is readjusted. Because of a novelty in the present situation, habitual coördinations are momentarily disturbed and a redirecting of one's movements is necessary. Ordinarily processes of perceiving do not supply the necessary controls. If the process of recognizing failed to develop under such circumstances, the resulting procedure would be one of trial and error, and the behavior of the human being would resemble that of an animal which is confronted by a novel situation.

On the next outstanding level of complexity in covert behavior is found the process of judging. A simple judgment, such as 'this is a table' is hardly more than a matter of recognizing. Indeed, all predications seem to be attenuated forms of recognizing which have been reduced largely to language habits. Our interest lies in the more complicated process, the comparative judgment. A full-fledged comparative judgment involves the associating together of two or more perceptions or objects. The first object is perceived, whereupon a kinæsthetic 'set' is carried over until the second object is perceived in terms of this 'set.' In the less attenuated forms of judging, imagery of some sort maintains this 'set' during the interval between the two perceptions, or it is the cue by which the 'set' is reinstated upon perceiving the second object. While the second object is being perceived, this 'set' is either altered or it remains unaltered. A recognition of this change or lack of change is the process of noting a relation between the two objects. Let us apply the term 'comprehension' to this recognition of a change, bearing in mind that any such change is dependent upon a motor 'set' and a resulting modification of the second perception by the first.

Comprehension, then is nothing other than a process of recognizing which is taking place in a complex situation. The situation is the factor which makes this recognition an act of knowing a concrete relation. The essential mechanisms of this knowing have been two sets of incipient motor adjustments, one by which a second perception is altered and one by which this change is identified. With repetition, however, comparative judgments revert to the simpler process of recognizing, as one's behavior cues become simplified.

To illustrate the functioning of comprehension in everyday behavior let us assume that the route from a man's home to his place of business includes, in one place, alternative paths. Long since, however, he has discovered which path is the shorter, and a simple sensory cue suffices to direct his movements over the shorter route. Suppose that one day he finds an alteration in the path to the right. Now the process of

discriminating becomes conscious. A problem now presents itself, but again one need not explicitly label the problem as such. A short period of hesitation may define the situation as a problem, thus: 'Which path is now the shorter?' A process of visual comparison, involving incipient motor 'sets' then takes place. The altered relation between the lengths of the two routes is noted and there takes place a consequent redirecting of one's movements. Once the shorter path is found, one's behavior reverts to less complicated forms of response.

Comprehending, then, is our third form of derived covert behavior; it is another form of the development of meaning. It is another form of recognizing. It is another means of bringing order into momentarily confused reaction tendencies but here the reaction tendencies are more complex than those previously mentioned. As in the case of the simpler form of recognizing, such a mode of covert behavior as we have called by the name *comprehension*, eliminates the necessity of a trial and error procedure in working one's way out of a difficulty.

Just as we found that the comparative judgment depends upon the act of associating together two or more perceptions, reasoning is an act of associating together two or more judgments. While judgment deals with situations or grouped objects, reasoning deals with problems or grouped situations. It relates situations together just as judgment relates objects to one another. We found that a full-fledged comparative judgment includes ideas and incipient motor 'sets.' Reasoning goes farther and includes concepts—imagery and incipient motor adjustments which apply to more than one situation. A concept is applied in responding to a novel grouping of situations. For example, suppose one has left his Ford facing up-hill on an incline. When he attempts to start the motor the engine fails to fire. He notes that the car is headed up an incline—a matter of judging. He applies the concept that a gravity-feed will not work up-hill when the gas is too low in the tank. He looks into the tank and finds that his supply is insufficient. This is a matter of judging, and also a

matter of recognizing the applicability of the concept which he has just recalled. Forthwith he decides upon the best way to solve his difficulty. He may find someone to help him turn the car around by hand or else he may hunt up the nearest gasoline supply-station. Let us give the name, 'interpretation,' to the act of recognizing an applicable concept when one is confronted by a problem. Here two new sets of incipient motor adjustments are involved. One has to do with the appearance of the concept in covert behavior; the other has to do with recognizing this concept as the applicable one. Here again there has developed the phenomenon of meaning and this meaning is a form of derived covert behavior which makes adaptive one's habitual behavior tendencies. Just as the comparative judgment includes the act of knowing a concrete relation, reasoning includes the act of knowing an abstract relation.

To illustrate reasoning in still another way, let us imagine that an individual is picking up a piece of ice for the first time. At once he perceives that it is cold. He picks up another piece of ice and likewise finds that it is cold. After several experiences of this sort he is led to anticipate the coldness before he handles the ice. In terms of imagery of coldness, verbal imagery, incipient tendencies to shake his hands as if they were cold and the like, he predicts that the next piece of ice will be cold. This prediction is the application of a concept to the present situation. One may go still farther, for there arises an implied problem. Will every piece of ice prove to be cold? He solves this problem implicitly, in terms of imagery and motor attitudes of assent or acceptance. 'Yes, every piece of ice is cold.' Such a procedure involves recognizing the applicability of a concept to a present problem. It is a process of interpreting.

We have said that one important rôle of covert behavior in life is that of redirecting overt behavior in times of temporary crises. When one sets up for himself a problem to solve and the answer is worked out at a desk or while sitting in an armchair, reasoning may not for the time being function to redirect some overt response which is suffering from confusion.

Yet, later on, there undoubtedly arises in the behavior of the individual some overt action or tendency toward overt action which shows the influence of this reasoning. At some later time a crisis is avoided because of this previous act of interpreting. In any event, the very process of setting up for oneself a problem to be solved is in a way confronting oneself with a temporary crisis. Alternative courses of thinking furnish a crisis in covert behavior to which the individual responds reflectively. It is derived covert behavior which terminates this crisis and solves the problem, providing the problem is not solved merely by the method of trial and error.

From the simplest act of localizing, found in connection with sensorimotor response, to the complex act of interpreting, found in connection with reasoning-motor reactions, we find no new or unique element in covert behavior—no new mental function. Association, attention, memory and motor expression are involved throughout, as well as sensation and image. In each level of response the individual has been doing essentially the same thing; he has been identifying labelling, localizing by means of sensory and imaginal contents on the one hand and motor adjustments on the other. In each level we find that derived covert behavior is synonymous with development of meaning.

The development of meaning, from its simplest to its most complex form, involves the phenomenon of projection. The more complex varieties of meaning are built upon the simpler varieties and retain this feature of the simple meanings, namely, projection and localization. A sense experience is projected or localized in space. Recognition involves the same sort of projection. It is the person recognized who is familiar; hardness is projected to the table; coldness is attributed to the ice. Features or characteristics are projected to objects and situations, in judging. This weight is heavier than that one. The light is very bright. This path is the shorter one; and so on. There is still to be found this projection, in reasoning. It is the gravity feed which will not work up-hill when the gas supply is low; forces actually seem to have power; laws are commonly regarded as ruling forces; meanings lie in problems, in situations and in difficulties.

XUM

In each of our four levels of derived covert behavior is to be found the process of abstracting. The incipient motor adjustments necessary for the projection and localization of sense experiences are products of selection. One's first coördinations, genetically speaking, are crude and awkward. The most useful and often repeated of these movements ultimately come to be used in localizing these sense impressions. Perception then rapidly becomes apperception, and the latter is a product of abstraction. Images come to function as ideas through the same mechanism of abstraction, and ideas become concepts in like fashion.

The image is involved in all of these four levels of sensorimotor activity. In simple perceiving, image-contents are usually blended with sensation to such an extent that the former are not noticed by introspection. In recognizing, however, imagery of some sort is always present and the definiteness of this imagery depends upon the degree to which the recognition has become attenuated by repetition. In judging, imagery is invariably present until the act becomes attenuated, and even then it does not entirely disappear from the final stage of judging,—namely, from the act of identifying or of labelling the concrete relation. Imagery appears in reasoning characteristically in the form of concepts.

Thus it turns out that the criteria of these different levels of derived covert behavior are not structural; they are not to be found in certain patterns of mental elements nor in certain stereotyped seriations of specific kinds of mental processes. Each level is identified by modes of behavior and by the situations to which these modes of behavior are responses.

We often perceive without definitely recognizing what we perceive. We may discriminate without consciously noting concrete relations. We may think in abstract terms without interpreting. We frequently behave as if we recognized, comprehended and interpreted but none of these latter activities is necessarily taking place. We act without knowing at the time what we are doing. Undoubtedly the higher animals behave in this fashion. They are conscious but not

conscious of things. For example a hawk, while soaring high in the air, perceives a mouse but presumably does not recognize it; the hawk does not know what it perceives. Instead, it defines a mouse by swooping after it. When a human being perceives a mouse, however, he defines or identifies it in terms of an attitude of familiarity; he may employ visual imagery of seeing mice in previous situations, or he experiences verbal tendencies to pronounce the word, mouse. The hawk's covert behavior is confined to simple perception and in any given instance passes over directly into instinct or habit while the human being's covert behavior begins with perception and aside from passing over into the possible overt reaction of killing the mouse, unfolds into incipient motor adjustments, *i.e.* to derived covert or intellectual behavior.

Again, a hen will discriminate when eating a mixture of cracked and whole-corn. It will run about the yard, apparently ignoring the cracked-corn, passing over nearer morsels for a piece of whole-corn farther away. To be sure the whole-corn offers the stronger stimulation; yet there is no doubt but that the hen sees the cracked-corn as well. While we have no reason for assuming that the hen knows what it is doing—it does not comprehend the relation between cracked and whole-corn, presumably,—yet it discriminates; it is revealing in its behavior a rudimentary form of judging.

Now let a human being be seated at a dinner-table on which there is a choice of foods. He labels the relation between one article of food and another. He identifies one as more pleasant, another as indigestible. He notes that this portion is larger than that one. If he is hungry he selects the larger portion. His response of eating is not only instinct and modified instinct as was the hen's behavior, but it is also a matter of reflection. The human being is discriminating but at the same time he is identifying what he is discriminating. This identification of concrete relations is carried out by means of derived covert behavior.

A bird-dog will become excited, jump about, bark and wag its tail vigorously at the sight of a gun. It will respond not

only to this gun but to that one, to a single barreled or a doubled barreled one, or in fact to a rifle. Evidently a primitive type of abstracting is here going on in the dog's behavior. It responds to the most often repeated features of guns, not to variable and differentiating features. Perhaps it is the general shape, the brightness of the barrel, perhaps the smell of powder or of metal; perhaps several such factors. In terms of habit, the dog is responding to a 'gun-situation.' In the genetic development of the human concept the same sort of thing is happening. One responds to repeated features of objects and ignores the variable features.

It does not seem far-fetched, then, to assume that in certain types of animal behavior are to be found the rudiments of reasoning. We do not wish to be anthropomorphic but merely desire to point out that the greatest difference between this precursor of reasoning in the dog and such reasoning as is found in the human being lies not in the presence of abstraction in the one case and its absence in the other, but in the circumstance that the dog does not interpret, while the human being does. The dog does not know what abstraction he is revealing by his behavior, while the human being knows, in the form of recognized concepts, what abstractions are occurring in his own behavior. In the dog's behavior the mechanism of abstraction is operating on the level of sensorimotor response and the product of this abstraction is revealed in the types of situations to which the animal responds. In human behavior the mechanism of abstraction operates on a more complicated and highly developed level, namely that of derived covert behavior and the product of this abstraction is a concept which is recognized. In a last analysis, however, the development of a concept in the human is habit formation taking place in the realm of covert behavior. Human abstraction functions as a cue not only to an overt response but also to a derived covert response. In other words, concepts are such imaginal cues as lead, in human behavior, to acts of knowing abstract relations.

Let us assume, then, that in animal behavior, covert responses are confined to perception, discrimination and a primi-

tive type of abstraction. Let us suppose that animal perception is a rudimentary form of recognition, that discrimination is a rudimentary form of judging and that responding to repeated features of situations is a rudimentary form of reasoning. If we compare human covert behavior with these types of response we find that the following elaborations have developed: To perception has been added recognition; to discrimination has been added comprehension and to abstracting has been added interpretation. In each case the addition has consisted of an incipient motor activity, superimposed upon the cruder types of overt response found in instinct and habit. This incipient motor activity consists of language habits, motor attitudes, and such non-language forms of thought as tracing objects with the eyes, hands, and fingers, gestures and the like. This development of incipient motor activity involves imagery. The value of this imagery for the incipient motor adjustments is the same as the value of sensation for instinct and habit. It is a coördinating mechanism. It is a preliminary stage of response whose end-stage is coördinated motor reaction. Imagery operates as a stimulus in derived covert behavior, or as a mediating mechanism between physiological stimulation and end-response, just as sensation operates as a secondary stimulus in instinct and habit, mediating between physiological stimulation and end-response.

Figure 1 illustrates the relation between covert behavior and overt response and also the relation between derived covert behavior and both covert and overt response. Let *S* represent external stimulation. *S'* represents proprioceptive stimulation. The concentric circles represent reflex arcs. The upper checkered region represents covert behavior and includes sensation, perception and the non-reflective stages of judging and reasoning, *i.e.* the phenomena of attention and association, discrimination, and abstraction. Such covert behavior functions in the control of instinct and habit. This control is indicated by the arrows, *B*.

Covert behavior, then, in any given instance, passes over directly into overt behavior. But this is not all. It also, in

the human being, passes over into incipient motor adjustments—attitudes, gestures, language responses and the like—or in other words into recognizing, comprehending and interpreting. Through proprioceptive stimulation these latter responses furnish meanings—a consciousness of objects, situations and relations. In this way such overt responses as are inadequately controlled by covert behavior, proper, are redirected or recoördinated by derived covert behavior. This latter type of control is represented by the arrows, *C*.

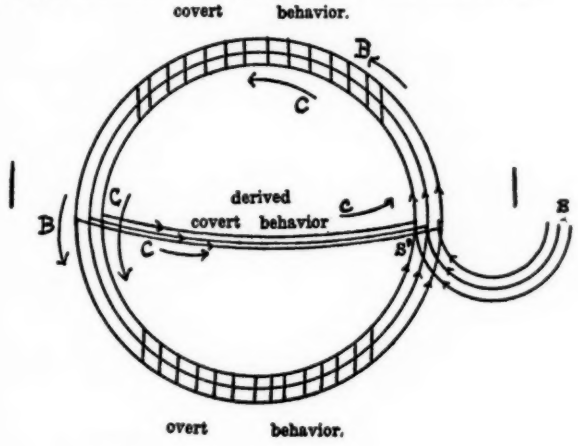

Fig. 1.

It may be noted in passing, that it is derived covert behavior which drops out during such processes of human learning as involve reflection in their earlier stages. Such habits do not become unconscious when automatized; they merely become non-reflective. Covert behavior is not eliminated, for perception is still involved.

If the scheme outlined in this paper be a correct account of the situation we can define intelligence as *derived covert behavior*. Its most rudimentary form is projection or localization, the next form is recognition, the next is comprehension, and the last is interpretation. Comprehension is the act of recognizing concrete relations and interpretation is the act of recognizing abstract relations. The rudiments of in-

telligence are possibly found in such processes of animal behavior as perceiving, discriminating and responding to repeated features of objects and situations. Our scheme implies that human beings may perceive, judge and reason unintelligently when such activities do not mature in any given instance to their elaborated stages of recognizing, comprehending and interpreting. It implies that intelligence is the act of knowing what one is doing while he is behaving. It implies that intelligence is something more than thinking or even more than thinking in abstract terms. Intelligence is the functioning of conscious meanings and the realization of values. There is here involved more than mere consciousness. There is involved *consciousness of* things.

From a genetic standpoint intelligence is an incipient motor activity whose typical antecedent is imagery and which has been superimposed upon the older and cruder type of instinctive response and modified instinct, whose typical antecedent is sensation.

Intelligence is subject to the same laws of modification and elaboration as instinct and is thus educable just as instinct is. Intelligence is a type of behavior which has assumed a directive influence over the older and more primitive form, thus giving to human beings a propensity toward self-determination. Intelligence might be likened to a catalytic agency, for, due to intelligence, the human being becomes an auto-governing mechanism.

To return to the question: 'What is intelligence, anyway?' we now have the following behavioristic answers: It is derived covert behavior. It is response to one's own covert behavior in terms of incipient motor adjustments. It is the development and operation of meanings. It is image plus motor attitude and language response. It is incipient adjustment which redirects overt behavior at the moment when instinct and habit do not suffice. It is the act of knowing what one is doing while he is behaving. Or in simpler language it is 'knowing'—cognition. It may be conveniently described by a reference to four outstanding levels of complexity, namely, projection, recognition, comprehension, and interpretation.